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SCIENCE IN THE SERVICE OF AGRICULTURE*

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Chief, Division of Plant Pathology and Botany, University of Minnesota, Agent, U. S. Department of Agriculture, and 1949 President, American Association for the Advancement of Science, Dr. Stakman has recently returned from a scientific mission to Japan. A group composed of five American scientists reviewed, under the auspices of the National Academy of Sciences, progress being made by Japanese scientists.

AGRICULTURE is fundamental to human subsistence, an obvious fact too often ignored by our lawmakers, educators, and the general public itself. A primary service that science must render to agriculture is to emphasize the importance and peculiar character of this basic industry, for indeed man is dependent on plant growth—on photosynthesis—for his very existence on earth. All his food and much of his raiment and shelter come either directly or indirectly from plants. Animals, important though they are, are essentially transformers of plant products and not primary producers. Modern industrial civilizations could not exist without the products of mines and factories, and man himself could not exist without the products of our fields and forests.

Because farming is a complex biological enterprise, subject to tremendous climatic and biotic hazards over which the individual farmer can exercise little or no control, a successful agriculture needs the services of all science. Government-supported science is especially needed because in most countries farming is carried on in many relatively small units. By its very nature, farming cannot be concentrated like processing and manufacturing. In the United States alone there are about 6 million farms distributed over an area of about 3 million square miles, with so many kinds and combinations of plants, animals, soils, and climates that nearly 500 types of farming can be recognized.

Hundreds of kinds of plants are extensively grown, and thousands more are grown to some extent. More than 27,000 Latin-named species and varieties are listed in Bailey's *Standard Cyclopedia of Horticulture*. Everyone knows something about varieties of apples, including differences in size, shape, color, texture, flavor, and keeping quality, and that there are numerous varieties of most other fruits, vegetables, and flowers. The differences between varieties of virtually all our cultivated plants are important both to the producer and to the processor, even though the bulk of consumers do not distinguish between them. Wheat is not simply wheat. There are a number of major botanical groups, such as the common bread wheats, the macaroni, or durum, group, the club wheats, and others. And there are smaller groups within the major ones. Within the common bread wheat group, for example, there are distinct market classes, such as hard red winter, soft red winter, and hard red spring, each with its peculiar growth habits and commercial uses. Within these classes, in turn, there are many commercial varieties; in 1935 the Department of Agriculture listed more than 200 varieties of wheat as being grown in the United States. Some of these may look very

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much alike but yield flour of quite different quality; some may look almost exactly alike but differ greatly in susceptibility to plant diseases. Varieties of flax may be similar in appearance but different in resistance to diseases or in amount and quality of oil produced. Discriminative choice and scientific applications play decisive roles long before farm products reach the market place.

Differences in varietal appearance and quality are so obvious in many plants as to be common knowledge; but few people realize that the grower must know the special soil and climatic requirements of the varieties that he grows and the special uses to which they may be put. Varieties often are suited only to very special soil and weather conditions, and the range of adaptability cannot be determined readily by the individual grower. There still is much to be learned about the physiology and pathology of a single kind of organism, and the problems often are complex and difficult of solution. Plant scientists must solve problems not only of many kinds of crop plants but those of dozens or hundreds of varieties within each kind. Animal scientists are confronted with similar situations. The almost bewildering complexity of plant and animal materials produced, and the wide variety of soil and climatic conditions that affect their production, obviously create numerous difficult problems for the scientist to solve.

Soil and climate are basic factors in agriculture because they determine the amount of food and feed that can be produced. Agriculture can be supplemented somewhat by aquiculture but only to a limited extent. Whether considered on a world-wide, nation-wide, or farm-wide basis, the soil is man's most valuable single asset. Unfortunately, its nature has too long been misunderstood, and it has therefore been lavishly squandered. The total amount of agricultural land in the world is neither unlimited nor inexhaustible. Intelligent land use, based on scientific facts and principles, is therefore of primary importance.

It is platitudinous to say that soil is extremely complex, physically, chemically, and biologically. And yet only those scientists who have studied soils intensively can realize how complex they really are and how rapidly they can deteriorate, or even disappear, under poor management. The formation of soils is a long process, including volcanic and glacial action, the weathering of many kinds of rocks, the effects of various kinds of plant growth, and the activities of myriads of microorganisms, often extending over centuries of time. And all too often man destroys in a few short years the soils that nature has so laboriously built by a long, slow process of evolution.

Soil conservation is one of the most important nation-wide problems in the United States. The present rate of soil deterioration in some of the richest agricultural regions is clearly a matter for alarm. This is no partisan or hyperbolic statement; it is based on official statistics and on extensive personal observations. The story of man's vandalism is written for all to see, not only in the older parts of the country but in some of the newer areas where the forest and prairies and plains have only recently been transformed into croplands, and still more recently into badlands. Nature's terrible drama of the dust bowl became so shocking a human tragedy as to force even the most apathetic into a realization that man cannot violate natural laws with impunity. The economic and sociologic consequences of a slower and more insidious debilitation of the soil were dramatized by the author of *Tobacco Road*. There are too many Tobacco Roads in the United States. Too many soils have lost their granular structure and have become as unproductive as so much cement. Water has leached out the fertility of many soils, and sheet erosion has carried the soil itself away from hillsides and the tops of gently undulating lands. One of the most distressing sights from an airplane is the endless succession of bald spots on many once-rich agricultural lands. Nature punishes man severely for his ruthless destruction of the natural vegetational cover that helped build and protect the soil. Thousands of acres of fertile fields have been so badly cut and gashed by gullies as to make them impassible to man and beast without bridging. In many areas gullies 10–100 feet deep have ruined thousands of farms within the lifetime of many of us.

Realism and common sense are beginning to replace the visionary optimism of the boomers and boosters who had the naïve conceit that land in the United States was unlimited in quantity and imperishable in quality. Time was when land was so plentiful that it could be exploited without endangering the national welfare. But that time is long past.

The pioneers who cut the forests, plowed the grasslands, drained the swamps, established the cattleman's empire, and discovered the sheepman's paradise aided in the phenomenal development of the United States. But they reckoned too little with the future and the penalties involved in the destruction of 50 million acres of cropland and the virtual destruction of an additional 50 million: 100 million acres of land—enough to feed and

clothe 50 million people moderately well. Soil scientists of the Department of Agriculture estimate that water and wind remove annually, from croplands and pastures, about 3 billion tons of soil, containing about 43 million tons of phosphorus, nitrogen, and potassium, which is fifty to sixty times as much as is replaced each year by commercial fertilizers.

We now have in the United States about 400 million acres of cropland, exclusive of grazing lands not suitable for cultivation—about enough to feed and clothe 160 million people with our present standard of living and productive efficiency. Much of this land is subject to erosion. Common decency to the nation and its future generations demands that everything possible be done to protect and preserve this national resource, and that it be done promptly and scientifically.

In 1936, in an address delivered at the 164th dinner of The New York Farmers, I made the following statement:

> Agriculture is a far more precarious undertaking than most other enterprises, because it is biological in nature, and various factors over which the farmer has almost no control can very quickly destroy not only the crops that he tries to produce but even the soil on which he produces them. A study of these factors and of the means of reducing their devastating effects is one of the principal obligations of research.
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> One of the primary needs is a body of facts to serve as a guide in a sane and sensible land utilization policy. Everyone knows that many good forest lands have been converted into poor farms, that many good range lands have been converted into still poorer farms, that many good swamps have been converted into nothing except fire and flood hazards. These marginal lands should either be taken out of production or the type of agriculture should be adapted to existing conditions. It sometimes is said that research, designed to help solve problems on these marginal lands, is wasted. But what about the people living on these lands? They are there. Are they to be abandoned to their fate, merely because they were misled into settling these lands by the need for increased agricultural production and the lure of high prices? We hear much about marginal lands and even about marginal farmers, to say nothing about marginal scientists. The criterion of what constitutes marginal land is a shifting one. When many of these lands were settled they were not marginal, because prices were high and it was possible to operate them profitably. They are marginal now, but will they be ten years hence? Whether they are then marginal lands, sub-marginal lands or above-marginal lands will depend partly at least on prices paid for what is grown on them. If they produce ten bushels of wheat an acre, they are sub-marginal if the price of wheat is only forty cents a bushel; but if the price is $1.25, they may be profitable. It is evident, of course, that extensive areas should never have been ploughed, because they yield good crops too infrequently altogether. But I want to protest against the scornful criticism that often is levelled against the people who ploughed them. Their mistake was in believing those who painted the lurid pictures of the new Canaans where milk and honey and gold flowed from the earth below and the sky above. Does land classification and zoning to prevent similar mistakes in the future transcend the legitimate functions of government and interfere too much with personal liberty? Mistakes were made, but the settlers themselves were not wholly to blame. In any case the problem is to correct those mistakes as scientifically as possible. Some readjustment is necessary, but it should be made as painless as possible.

The concept regarding marginal lands was not long in changing. The tremendous demand for food and other agricultural products for ourselves and other peoples stimulated efforts to make all lands yield to maximum capacity, even at the risk of serious soil impoverishment. Phenomenal wartime agricultural production was attained partly because of providential weather, for which man can take no credit, and partly because of the industry of the farmer and his intelligent use of the tools given him by science and technology.

World War II led to stocktaking of past progress and present agricultural and industrial potential. For the present, the result was reassuring. As concerns agriculture, it seems clear that man's constructive efforts can counterbalance his destructive practices if he puts common sense, science, and technology to work. Comparison is made between the two world wars for the obvious reason that in both every attempt was made to utilize all available knowledge, skills, materials, and technics to attain maximum production.

During the war years 1942 to 1945, inclusive, American farmers produced about 2 billion bushels more corn than would have been possible during four years of World War I; in a single year of World War I stem rust destroyed about 300 million bushels of wheat in the United States and Canada, but losses were comparatively small during all of World War II; it is estimated that the efficiency of animal production during World War II was 25 percent higher than in 1919; the use of farm machinery in World War II released enough land previously used for work animals to feed 16 million cattle or 26 million hogs, so badly needed for dairy products and beef and pork; an idea of the number of men released for other types of national service by the mechanization of farming can be gained from the fact that in 1900 the production of 100 bushels of wheat required 108 hours of man labor and only 47 hours in 1940. This progress was made possible almost entirely by utilizing the results of research and invention.

It is impossible—probably even undesirable—to try to apportion credit to the various guilds of

scientists and technologists who accumulated the facts, produced the materials, or elucidated the principles that helped make the truly marvelous agricultural production of World War II possible. True it is, however, that the production of superior varieties of crop plants, the selection of better breeds or lines of farm animals, wider knowledge and better practices with respect to soil conservation, fertilization, and tillage, greater attention to the principles of plant and animal nutrition, more efficient control of plant and animal diseases and pests, and more extensive use of farm machinery all contributed their share. Important also is the fact that more knowledge had accumulated regarding the prevention of deterioration of foods and feeds and better methods had been devised for processing and preserving them. It is impossible to discuss in detail the contributions made in each of the various fields; only a few examples can be given. The resulting lack of balance is unfortunate but almost unavoidable.

Corn, wheat, and oats are outstanding examples of crop plants that were so improved as to contribute greatly to agricultural production during the war years. There are many others, but these illustrate progress attained through the application of the principles of genetics to plant improvement.

Without hybrid corn there could have been no 3.2-billion-bushel crop in 1946. This corn helped feed 80 million cattle, more than 44 million hogs, and enough chickens to lay 55 billion eggs. The 600 million extra bushels that go to the credit of hybrid corn in 1945 and the 2 billion during the war years constitute an astonishing record. But more amazing is the scientific record, especially the rapidity of the progress after the underlying principles were developed and applied. Experiments in crossing varieties of corn were made as early as 1881, but inbreeding experiments were begun as late as 1905, and the crossing of inbred lines about 1908. As corn is normally cross-pollinated, there was a popular belief that self-pollination inexorably led to deterioration. Indeed, selfed lines did decrease in vigor, but it was found that surprising results could be obtained by first selfing lines to attain relative uniformity and then crossing them in various combinations. Neither inbred parent amounted to much. But they were good parents: they complemented each other in such a way as to produce superior offspring. The process was carried further, and double crosses were made. As an example, selfed lines A, B, C, D are used as parents; A is crossed with B, and C with D. Then $A \times B$ is crossed with progeny of $C \times D$. It sounds simple, but not all combinations are good. It is estimated that of the 30,000 lines developed by 1939 only 2.4 percent were useful in corn improvement. Some, however, were very good. Corn breeders have learned how to use the results of single cross tests for calculating the potential yielding ability of double crosses, thus enabling them to emancipate themselves from much of the laborious testing that was previously necessary.

Information basic to the development of hybrid corn was originally obtained as a result of natural curiosity regarding the effects of inbreeding. The scientific study of corn genetics and its practical application in breeding followed. The results have enriched the science of genetics and the agriculture of many countries. In the United States only 0. percent of the corn acreage was planted to hybrids in 1933; now it is almost 100 per cent in the corn belt states, with an average increase of about 2 percent in yield over that of the open-pollinated varieties grown 15 short years ago.

In the interim between the two world wars the terribly destructive stem rust of wheat was brought under at least temporary control in the hard red spring wheat region of the United States and Canada through the use of newly produced rust-resistant varieties and the eradication of rust-susceptible barberry bushes. It is true that there were extremely destructive epidemics in 1935 and 1937 because of a series of extraordinary meteorologic events that enabled unusual amounts of rust to spread northward from Texas, and because of the appearance of a new parasitic race of the rust that ended the short but useful career of the best spring wheat variety then grown. Other varieties were ready, however, to meet the new menace, and they helped make the badly needed billion-bushel wheat crops possible. The wheat varieties of World War I never could have equalled that performance. The production of these rust-resistant varieties was made possible only because plant breeders and plant pathologists had learned enough about the genetics of the wheat and of the rust fungus to proceed intelligently in the work of breeding and testing.

The oat varieties of World War I could not have performed as did those of War II, even though the latter barely survived the war. A number of superior varieties were obtained by crossing the varieties Richland and Victoria. Richland contributed stem-rust resistance, and Victoria contributed crown-rust resistance. These new varieties became extremely popular in most oat

growing states in the early forties because they often yielded 20–50 percent more than the varieties that they replaced. Moreover, they seemed to have eliminated the disease hazard. They served well until the end of the war. Then their demise was almost as rapid as their rise. They are now doomed to oblivion because of their extraordinary susceptibility to a disease that was unknown until they became widely grown. There is genetic linkage between the factors for crown-rust resistance in Victoria oats and those for susceptibility to the new disease, which simply means that if Victoria contributes crown-rust resistance when crossed with other varieties it also contributes susceptibility to the new disease: the bad goes with the good. But breeders and pathologists had been crossing still other oat varieties because they knew that the Richland-Victoria group of hybrids was menaced by parasitic races of the stem-rust fungus that increased in prevalence as the acreage of the new varieties increased.

The varieties White Tartar and Bond contributed rust resistance to the newest varieties that are grown today. Although now performing well, these new varieties are known to be susceptible to some parasitic races of the rust fungi that are present in the United States in only a few regions and in small amounts. As there is precedent for the assumption that these races of fungi may become more widespread and prevalent, still more crosses are being made in an attempt to meet the menace if it materializes.

And so the succession of new varieties continues: New conditions, new varieties, which may in turn bring new problems along with their new values. The improvement in varieties of almost all important crop plants through the application of scientific principles during the past half century has been almost miraculous. Not only have acre yields and quality improved, but the dangers of violent fluctuations in production have been diminished through the development of varieties that resist plant diseases and insect pests and that are better adapted to the soils and weather in the regions in which they are grown.

Science has given agriculture hundreds of improved varieties of plants within the past few decades. But these varieties cannot perform to their maximum capacity unless grown on suitable soils and under appropriate climatic conditions. There is little use in attempting to grow a good variety on bad soil and under weather conditions to which it is not suited. And so we come back to the soil. Without soil there are no plants; without good soil, even good varieties cannot thrive; and on really bad soil even good varieties are likely to be bad; consequently, soil scientists must provide the information requisite to the proper nutrition of the improved varieties.

It is common knowledge that some plants, such as clover and alfalfa, will not grow well on acid soils and that certain kinds, such as blueberries and cranberries, like an acid soil. Well known, also, is the fact that all plants require nitrogen, phosphorus, potassium, calcium, and certain other nutrient elements and that they must be available in proper proportions. Progress is continually being made in determining the peculiar requirements of various kinds of soil and plants.

Plants, however, also require minute quantities of boron, copper, sulfur, manganese, zinc, and certain other elements for normal growth. Only a few parts in a million parts of soil may be necessary, but sometimes they are very necessary. The causes of many mysterious and destructive deficiency diseases of plants have been revealed by increasing knowledge regarding the role of these minor elements. Thus, a dry rot of sugar beets and certain other plants is now known to be caused by lack of boron and can easily be prevented by supplying it to the soil. Likewise, the internal cork of apples, internal browning of cauliflower, top rot of tobacco, and cracked stem of celery can all be prevented by adding small amounts of boron to the soil. A slight excess, on the other hand, is deleterious. Deficiency of copper causes wilting of the upper leaves of tobacco and unthriftiness of certain other plants. The addition of small amounts of manganese to certain soils makes the difference between a good crop of tomatoes and virtual crop failure. Deficiencies of iron, copper, and cobalt in soils are known to retard the development of cattle that feed on plants grown on such soils. Until recently these minor elements were not added to commercial fertilizers, but now, with more precise information, attempts are being made to add them in the proper amounts. The complicated relationships between the minor elements and soil productivity and the effects on plant growth and animal nutrition are strikingly depicted in a book compiled and published by the Chilean Nitrate Educational Bureau, Inc., New York (*Bibliography of the Literature on the Minor Elements and their Relation to Plant and Animal Nutrition,* 4th ed., Vol. I. Pp. 1,037). The abstracts were originally obtained from scientific journals and have been put into one volume for the convenience of investigators. Even casual perusal of the titles and contents of the 10,000

abstracts indicates the complexity of the problems, the magnitude of the work involved in attempting to solve them, and the prodigious amount of information that has already been obtained.

Minute quantities of certain elements in soils may be poisonous to plants or animals. Among them are arsenic, selenium, thalium, and molybdenum. The mystery of the so-called alkali disease is now solved. Beginning about the middle of the nineteenth century, it was observed that animals in certain areas of the range country of western United States became debilitated and often died in large numbers. Many causes were assumed, among them alkali poisoning. It was not until 1928 that it was found that the disease appeared when animals ate grain or forage produced in certain areas. This led to the discovery that the poisonous vegetation contained small amounts of selenium, that concentrations of one part per million might kill animals, and that some plants take up the selenium from the soil and others do not. The selenium-bearing soils are dangerous when there is not enough rainfall or irrigation water to wash out the selenium.

The soil is a vast chemical and physical laboratory and a beehive of microbial activity. Bacteria, fungi, protozoa, and other forms of plant and animal life help make the soil what it is. Microorganisms decompose plant residues, thus furnishing humus; some enrich the soil by fixing atmospheric nitrogen, either as symbionts in the nodules on roots of legumes or as free-living forms; some parasitize the roots of higher plants. There are many kinds, with many kinds of interrelationships. A struggle for existence is going on continuously, with consequent shifts in the relative numbers of each kind of organism. Some are mutually beneficial; others are antagonistic. The total and relative numbers are affected also by soil fertilization, cultivation, and other tillage practices. Soil microbiologists are continually studying the activities of these microorganisms and finding out what can be done to promote the activity of those that help make soil a suitable medium for plant growth.

Even when the best varieties are grown on the best soils, crop plants still are subject to the destructive effects of unfavorable weather, insect pests, and plant diseases. There are violent fluctuations in total production of many crops because of winter injury, droughts, floods, hail, and heavy winds. The principal food crops of the United States are grown in a region in which the weather is continental, with extremes of temperature and wide variations in rainfall. In the upper Mississippi Basin an annual range of 150° in temperature is not uncommon. In this area, the range in a single state has been as great as 160°. Summer temperatures of more than 100° F. are common in many areas. Such temperatures, especially when combined with low humidity and high wind velocity, often cause severe injury to the most heat- and drought-resistant plants. Extremely low winter temperatures under some conditions may cause severe and extensive winter injury to fall-sown grains and perennial forage crops. The terrible drought years of the 1930s created not only visions of deserts but actual deserts. Small wonder that acute problems of agricultural economics and sociology result from such violent fluctuations in production. The annual production of wheat in Kansas has varied from more than 200 million bushels to less than 60 million; that in North Dakota from more than 150 million to about 20 million; that for the country as a whole from more than a billion to less than half a billion. In 1932 we produced almost 3 billion bushels of corn; in 1933, less than 2.5 billion; and, in 1934, only about 1.5 billion. In 1946 the production was about 3.2 billion bushels, but bad weather reduced it to 2.4 billion in 1947.

Although science has not yet learned to control weather, it has helped reduce its destructive effects by providing better-adapted varieties of plants and better tillage methods. The substitution of sorghums for corn in the drier parts of the Great Plains, and of hardier varieties of wheat for less hardy varieties in much of the Great Plains area, has helped greatly to ensure production. The continual introduction or production of better winter-hardy and drought-resistant grains, perennial forage grasses, and legumes is helping to ensure and stabilize production. Even the production of nonshattering varieties of grain has reduced grain losses and made possible the use of combines, which cut and thresh the grain in one operation, thus saving a great amount of labor. The development of varieties that resist unfavorable weather has been of value to production even in those areas where weather is not usually severe. This, and the development of soil-management practices appropriate to the region, have made possible the expansion of agriculture to the western belt of the Great Plains area, thus making millions of additional acres available for producing foods and feeds.

Insects, like weather, may cause devastating damage in a short time. Hordes of grasshoppers have often ruined the crops over wide areas. Chinch bugs, the Hessian fly, the cotton boll wee-

vil, the Japanese beetle, and many other insects can debilitate or destroy plants on a large scale. Great progress has been made in efficiency and economy of controlling many of these insects. The cotton boll weevil, for example, which almost ruined the cotton industry in large areas of the South, has been brought under reasonable control by a combination of suitable varieties and airplane dusting with insecticidal dusts. Varieties of wheat have been produced that are far more resistant to the Hessian fly and chinch bugs than those formerly grown. There has been a remarkable record of achievement in the production of more effective insecticides, such as the well-known DDT and others. But many of these insecticides bring with them new problems, for they may be injurious to certain kinds of plants and may poison the soil when used in large quantities. They may even be dangerous to man when not completely removed from fruits and vegetables. There still is urgent need for extensive research on the better control of insects.

At best the situation with respect to insects is bad enough, but new ones are periodically introduced into the country despite the best protective efforts of Federal and state plant-quarantine services. The introduction of the Japanese beetle and the European corn borer are recent examples of destructive insects that were imported into the United States by man. Scientists in general realize the dangers of introducing new insect pests, and governmental agencies are doing what they can to prevent it. Public apathy, ignorance, or perverseness, however, often nullify their efforts. There must be wider appreciation of the importance of the plant-quarantine regulations.

Plant diseases caused by viruses, bacteria, and fungi are a continual menace to all kinds of crop plants. Some of the most destructive and typically epidemic, such as the rusts of wheat and other small grains and the late blight of potatoes and tomatoes, can spread with frightening rapidity and devastating effect.

As an example, stem rust of wheat and other grains and grasses is caused by a fungus that multiplies by means of several kinds of spores. Those that spread the disease from wheat to wheat are cylindrical in shape and about 0.001 inch in length. On a single acre of moderately rusted wheat there are about 10,000 billion of them, and they can be carried far and wide by the wind. During a period of moderate to strong south winds in 1925, countless numbers were blown northward from Texas, Oklahoma, and southern Kansas and in a week infected an area of 250,000 square miles. The individual farmer is helpless to prevent the spread of these invisible enemies, and so is everyone else. About 40 years ago, plant scientists, therefore, began an intensive search for resistant varieties. Extensive testing and breeding work was done, and a number of resistant varieties were produced, only to lose their rust resistance in some places and at certain times. Intensive study of the rust finally revealed the fact that it was carrying on an extensive breeding program of its own and that there are varieties of stem rust just as there are varieties of wheat, except that the rust varieties are microscopic in size and therefore harder to recognize. Later it was found that within the varieties there are rust races that can be distinguished only by their parasitic effects on varieties of wheat and other grains. More than 200 races of the wheat variety of stem rust are now known. A wheat variety may be immune from some races, moderately resistant to others, and completely susceptible to still others. As the prevalence of races may vary from place to place and from year to year, a wheat variety may be resistant here but not there, and then but not now. Worse still, new races are produced continually by a process of hybridization between existing races, for the rust fungus, like wheat, has a sexual stage. Fortunately, the sexual stage can develop only on a relatively useless plant, the common barberry; hence the barberry-eradication campaign, which has reduced the number of races and delayed the onset of rust early in the growing season in the northern half of the United States. As rust can exist independently of barberries in certain areas of southern United States and Mexico and then spread northward in the spring and early summer, barberry eradication must be supplemented by the use of resistant varieties of wheat and other grains to attain practical control of the rust. It has, however, been impossible to eradicate all barberries, and numerous new rust races are continually being produced, especially in some of the eastern and northeastern states and in certain other areas where barberries are still numerous. Some of these races are much more virulent than those now prevalent in grain-growing areas. It is possible, but not certain, that some of these virulent races may increase, become established, and attack varieties that are now resistant. Accordingly, plant scientists are trying to prepare against possible future emergencies by making extensive series of crosses in an attempt to produce varieties that can resist all races of rust now known, whether important at present or not.

Crop plants are continuously threatened by myriads of pathogenic organisms. There are thousands of species, varieties, and races. More than 3,000 species of plant rusts are known. Each species of pathogen may comprise numerous races, and new ones are continually being produced by natural processes of mutation and hybridization. Even though only a relatively small percentage of the new ones may be more dangerous than those now in existence, past experience shows clearly the imperative necessity for continually being on the alert to detect evidence of the presence of new diseases or new forms of old ones, so that they may not become established before science has provided the basis for controlling them.

Despite many difficulties, great progress has been made in controlling plant diseases. The production and utilization of resistant varieties of plants, even though they may be of only temporary or regional value, have greatly reduced the danger of catastrophe from plant-disease epidemics. Moreover, many diseases can be controlled at least fairly well by appropriate crop rotations, the treatment of seeds and other propagative plant parts with fungicidal chemicals, and the spraying or dusting of growing plants with similar substances. During the past 30 years improvement in fungicides has paralleled that in insecticides. Merely as one indication of the value of devising proper seed disinfectants and methods for applying them is the fact that the treatment of seed corn to prevent the rotting of seed and the development of seedling blights and certain other diseases sometimes results in almost as great an increase in yields as that resulting from the use of hybrid corn.

Man himself has often changed plant-disease situations to his own detriment. Some of the most destructive diseases that occur in virtually every country of the world are there because man brought them there. He brought them on the propagative parts of plants, on packing materials, and in innumerable other ways. The ravages of the chestnut blight, white pine blister rust, citrus canker, and Dutch elm disease in the United States are a few spectacular examples of the danger of introducing into one part of the world pathogens relatively unimportant in their native habitat, which became extremely destructive when taken to a new climatic or biotic environment. Experiences with imported plant diseases strongly reinforce what has been said in connection with insects about the urgent necessity of adequate plant quarantines.

When once soil, weather, and man have combined to produce good plants, as scientifically as possible, the problem of the proper utilization of the plant products still remains. As animal production is one of the most important phases of agriculture, the methods of conversion of feed and forage into meat and milk and other animal products should be as scientific as possible. To detail the progress that has been made in animal nutrition would be impossible in an article of this length. Efficiency is dependent partly on the breeds of animals used and on the way in which they are fed. No one needs to be reminded that the scrub cattle on scrub pastures of a few years ago were considered to be doing well to produce 800 pounds of beef in 3 or 4 years. With the modern beef breeds, a steer is expected to attain a weight of 1,000 pounds in 12–18 months, depending on how and what he is fed. It is estimated that the efficiency of animal production has increased about 25 percent since the end of World War I. This is due principally to animal improvement and more scientific feeding.

Continual progress is being made in learning more about the nutritional requirements of different kinds of farm animals and the reasons why feed or forage from certain regions often gives better results than that from others. Phosphorus deficiency is a fairly common disorder, and the addition of 6–14 grams of phosphorus a day to feed for calves and cows in southern Texas has had remarkable results. Animals, like plants, require some of the minor elements and sometimes suffer from deficiency. Copper, cobalt, and manganese, for example, are all needed. Cobalt deficiency is not uncommon in some areas of the United States, particularly in the southeastern states, certain localities of the Lake states, and in the Northeast. The addition of one ounce of cobalt sulphate to each hundred pounds of feed has relieved emaciated cattle from their unthrifty condition. Scientific feeding of animals is becoming more and more scientific.

Evidence is accumulating that crossbreeding of animals may result in improved vigor and quality. Progress is slower in breeding animals than with plants, but progress so far made with beef cattle, swine, sheep, dairy cattle, and poultry shows clearly that desired combinations of qualities can be obtained. New breeds of pigs made to order for lean bacon, or for lard, or for hams of a certain size already have been produced. The Columbia breed of sheep has been developed by the Department of Agriculture as a result of crossbreeding between the Lincoln and Rambouillet followed by inbreeding to fix the type. The new breed combines the mutton- and wool-producing

qualities of the two parents into a single dual-purpose animal. Crossbreeding Zebu and Aberdeen Angus cattle to produce animals that can thrive in the subtropical climate of the Gulf states is in progress. Hybrid corn; why not hybrid animals?

One of the great advances in animal production has been the development of methods of artificial insemination so that the beneficial influence of superior sires may be more widely diffused.

Increased knowledge of animal diseases has kept the disease hazard within reasonable limits in the United States for a number of years. Hog cholera, which in 1897 killed one pig in seven in the United States, is still present but is now controlled by vaccination. Other diseases, such as anthrax, brucellosis, swine erysipelas, tuberculosis, and mastitis, have been kept under reasonable control by a combination of sanitary procedures, the use of vaccines, and of drugs such as the sulfa drugs and penicillin. Foot-and-mouth disease, which appeared in limited areas in this country in the past, has been eradicated and then kept out by appropriate quarantine measures. It has, however, been introduced into Mexico recently and has spread rapidly. Whether it will be introduced into the United States again cannot be predicted, but every effort is being made to exclude it. Veterinarians are of course confronted with new situations resulting from the appearance of new diseases. One example is the destructive Newcastle disease of poultry, which was first found in the United States in 1944. How it got here is not known, but extensive research is being done in an attempt to devise satisfactory control measures.

An attempt has been made to give some idea of the service of science to agriculture, and to indicate especially progress between the two world wars. The complete potential of a country may be estimated, but it is seldom used in a country like the United States except in times of extreme stress. Just as science serves agriculture, so must agriculture serve the nation. As one phase of national preparedness, we must make sure that agriculture is in sound condition. Science should therefore help to formulate agricultural policies based on facts instead of fancies, on sound judgment instead of hysteria and prejudice. Science in the service of agriculture may help provide perspective in attitudes and actions. The following, written in 1936, is pertinent:

> A few years ago we heard much about the increasing difficulty of producing enough in this country. The population was increasing rapidly because of immigration and a relatively high birth rate, industrialization and urbanization were proceeding rapidly, the standard of living was rising, and it seemed that there might soon be too many mouths to feed. The doctrine was preached that we might soon be pressing heavily on means of subsistence. Then, with kaleidoscopic suddenness, there came a change. We heard we were producing more than we could consume, that agricultural production had outstripped consumption. Agricultural scientists even were blamed for having shown how to increase the efficiency of production without having considered what was to be done with the increased amount that was produced. We ploughed cotton under; we killed pigs; we took land out of production, and many popular prophets wanted to take agricultural scientists out of production, too. Then came bad seasons: Drought, insect plagues and plant disease epidemics reminded us forcibly that farming deals with living things and that there are tremendous climatic and biotic factors before the destructive fury of which mere man is pathetically helpless. A farm is not a factory; it is a place where living things are encouraged to grow. And nature, not man, is the final arbiter as to the amount we will produce. Man has not yet completed his conquest of nature; nature can upset man's little calculations with astounding suddenness. Now we again hear little about overproduction but we hear much about the necessity of maintaining reserves. First the country is glutted with people, then it is glutted with food, and I suppose it is always glutted with talk, including mine. Have opinions merely changed or have situations changed? Both. (164th dinner, The New York Farmers.)

What changes in a decade! It will be interesting to continue the statement ten years hence. More science is urgently needed; and ethics could help, also.

To avoid diffuseness and glittering generalities, this discussion has dealt largely with the importance of agriculture in the United States. What has been said has equal relevance to international questions in agriculture. Can we develop a scientific and ethical attitude toward global problems of human subsistence? Until we do we can hardly expect to see the emergence of One World. And if we wish to attain this goal we shall have to face facts and alleviate desperate situations with scientific realism and true justice.

A CONTINUED SEARCH FOR THE BEGINNING

WM. T. SKILLING

Drawings by Frances W. Wright, of the Harvard Observatory

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KANT'S NEBULAR THEORY—1755

AN EARLY attempt to account scientifically for the sun and its family of planets was made about the middle of the eighteenth century by the great German philosopher Immanuel Kant. This was the first of such theories to seem plausible enough, and to be worked out in sufficient detail, to attract much attention.

The keen, analytical mind of Kant started with a great unorganized nebula at rest in space. An English book published a little earlier by Thomas Wright had suggested that the solar system might have resulted from a nebulous mass of matter such as the telescopes of that day revealed in some places in the sky. This thought, though it received little public attention, acted as a trigger to Kant's lively imagination. He thought out the steps by which it seemed probable to him that a nebula might change to a family of planets, with a sun at their center, and published his theory anonymously under the title *Natural History of the Universe*.

The Philosopher was not trained in physical science, but he knew the more common laws of chemistry and physics. He pictured the molecules of gas as being drawn to each other by chemical and gravitational attraction. This would set up motion in the mass, and the smaller particles would be drawn into combination with the larger ones. Little clusters that formed in this way would grow larger and larger, as snowballs increase, until finally the sun and the planets would be formed.

Kant's theory was in violation of laws of dynamics that forbid the developing of itself of any circulatory motion like that of the planets from a nebula originally at rest. Even if such a system possesses some circling motion to begin with, the amount of such motion (the "moment of momentum") can never become more nor less without help from the outside.

Kant's a priori reasoning did not stop with a consideration of inert matter, for he pictured man as developing on other planets than the earth. He thought the outer planets, being formed first, would have a higher type of life than the earth. He apparently did not take into account the fact that a planet so remote from the sun as Saturn, the outermost one then known, would receive only about 1 percent as much heat as the earth does, and in consequence its temperature would make life as we know it impossible. The natural state of water on Saturn would be the solid form, and liquid water would be as much a novelty as liquid lead or iron is with us.

Kant's theory never produced much effect except in the nature of opposition to it. He was a keen-minded philosopher, but he was not a physicist.

THE NEBULAR HYPOTHESIS OF LAPLACE—1796

Nearly half a century after Kant published his discussion of a nebular origin of the sun and planets, Laplace, the greatest of French mathematicians, also starting with a nebula, proposed a theory that captured and held the field of cosmogony for a hundred years. In some respects Laplace's nebular hypothesis, as it is called, was similar to that of Kant. But he assumed that the original nebula from which the solar system developed had already some rotary motion. This would explain the fact that the planets now all revolve around the sun in the same direction and nearly in the same plane.

Although Laplace was both a mathematician and a physicist, he admitted that his hypothesis was based on neither observation nor calculation. It was originally published rather obscurely in an appendix to a popular book on astronomy. Nevertheless, there have been few advances in science that have aroused so much general interest and valuable discussion. Kant's theory had not only laid the groundwork for Laplace's theory, but the discussions of it prepared the public mind for accepting the theory of Laplace.

Laplace's nebular hypothesis did indeed help to explain most of the peculiarities of the solar system. The heat of the sun, for example, could be accounted for, as Helmholtz pointed out in 1854,

by the gradual increase of pressure as the matter within the widespread nebula contracted. Contraction against internal pressure produces heat.

The chief new points in Laplace's theory as compared with that of Kant were: (1) Laplace started with a nebula of very high temperature, slowly rotating. The loss of heat by radiation caused it to contract, and the motion of the planets around the sun could be accounted for. (2) Laplace assumed (without careful analysis of the matter) that as the mass contracted a condition would be reached at which centrifugal effect at the equator, working against contraction, would just balance and offset the inward force of gravitation. He thought that on further contraction of the inner mass a ring would be left behind and that the part within the ring would continue to shrink and leave a space between the ring and the main mass within. He assumed that in the course of time the ring would collect into a planet. After further contraction, another such point of balance would be reached, and a ring, later changing to a planet, would form; and so the process would go on until all the planets were made and the rest of the material had condensed to a sun at the center.

Two objections to this assumption of rings are now known: first, that such a ring, if gaseous, would dissipate rather than solidify into a planet, for the expansive force of the gas would be greater than the power of so small a mass to hold it by attraction; second, after a ring began to form it would be continuous instead of a succession of rings. If the rings were made of solid particles instead of gas, the result would be a thin disk, like Saturn's ring. (The gaps in Saturn's rings have been supposed to result from the disturbing attraction of Saturn's satellites.)

Another fatal error in the nebular hypothesis is found in the quantity of circular motion ("moment of momentum") of the planets as compared with the motion of the sun rotating on its axis. There is no way of explaining why the planets, which have only about one seventh of 1 percent of the total mass of the solar system, have about 98 percent of this quantity of motion, whereas the immensely greater sun has only 2 percent. (The quantity of circular motion of a body is the product of its mass, the velocity at which it moves, and its distance from the center, *mvr*.)

The rock upon which the nebular hypothesis was finally wrecked was one of the very arguments that had been considered a strong point in its support, the fact that the heat of the sun could be accounted for by compression as it contracted. But this argument failed, for it gave a method by which the entire time that the sun had been shining at its present rate could be calculated. It placed the too-short limit of 25 million years on the past lifetime of the sun, assuming that compression was its *only* source of heat—and no other source was then known.

THE PLANETESIMAL HYPOTHESIS OF CHAMBERLIN AND MOULTON—1900

In 1899 T. C. Chamberlin, head of the Department of Geology at the University of Chicago, became convinced that geological evidence gave proof that the earth itself, to say nothing of the sun, is far older than this limit of 25 million years. (It is now inferred from the character of radioactive rocks that the earth has been at its present size for 2 billion years.)

In 1900 Chamberlin and his associate, F. R. Moulton, in the Astronomy Department of the University of Chicago, undertook a thorough analysis of the nebular hypothesis to determine whether it gave a satisfactory explanation of the known facts relating to the age of the earth. It was found that there was no way in which the present system of planets could have acquired their great rotational momentum leaving in the immensely more massive sun only enough to account for the present slow rotation of the sun. They concluded that some outside influence was necessary to explain the existing revolutions of planets and rotation of the sun. They pictured a passing star as furnishing the disturbing force of attraction that resulted in the present system of sun and planets—that drew them from the sun and endowed them with rapid motion in nearly concentric orbits around their still slowly rotating central sun.

As such a star approached the sun from a great distance it began to raise a tide in the gaseous material of which the sun is composed, as the moon raises a tide in the ocean. The tide would mount higher and higher as the star came nearer, and its growth would be aided by the eruptive forces in the sun that now cause prominences hundreds of thousands of miles above the sun's surface. These star tides would be far greater than our moon tides, for a star as great as the sun would have 27 million times as much mass as the moon and correspondingly more attractive force at any given distance.

As the star swept by the sun, at perhaps no greater distance than some of our planets are now, the material drawn out toward it would be attracted by the star and so would be given the cross-motion with reference to the sun that the planets have. The orbital motion of the planets would thus

be accounted for. But, falling behind the fast-moving star, the scattered material of all sizes of particles, from molecules to partly grown planets, would be drawn around the sun in obedience to the sun's gravitational force. The visiting star would go on its way, doubtless taking with it material similarly drawn out from it by the sun, which it could use for making a family of planets. Long ago it would have become indistinguishable, as seen from the earth, from any other star. The larger masses of matter going around the sun would sweep up the smaller particles and grow to be the planets, in the course of time, as they now are.

This theory was called by its authors "The Planetesimal Hypothesis," from the word "planet" and an ending that suggests the various sizes of the innumerable particles composing the material as it was when first drawn off from the sun. Chamberlin and Moulton in using the word "hypothesis" indicated that the great question regarding the origin of planets has yet to be settled beyond question.

Among others who have suggested modifications of the two-star theory, Sir James Jeans, in 1918, proposed his somewhat different version of the same story under the title "The Tidal Theory." The visiting star of Jeans' tidal theory came at an earlier time, when the sun was vastly more expanded, and he emphasized the effect of tides rather than of solar eruptions.

Then, too, H. N. Russell, of Princeton University, criticized the entire theory that material so drawn off from the sun could have condensed into planets that would have remained with the sun. Lyman Spitzer, following this thought, has shown that if material to make the planets were removed from the sun some of it would have come from a depth at which its temperature would have been around 10,000,000° F. He found by computation that such highly heated gas raised suddenly by a passing star out of the sun, where the sun's powerful gravitational force had kept it from expanding, would, upon release, expand with terrifically explosive effect. Most of the gas would have been blown entirely away from the region of the solar system, and what was left would be in the form of a tenuous gas that could hardly be expected to condense into planets.

By way of illustration, if we will merely consider an auto tire blowout, where a break in the casing gives the air room to expand, we can get some faint conception of an explosion of gas at millions of degrees, suddenly lifted out of the sun and given elbow room to expand. The break in the rubber would only reduce the pressure on the air within from about three atmospheres to one. Pressure in even the surface of the sun is caused by a gravitational force 28 times as great as the earth's downward pull. The temperature of the air in the tire causing its outward push is only about 300 centigrade degrees above absolute zero; compare the energy of gas at such a temperature with the energy of solar gases at thousands or millions of degrees. We scarcely need computation to realize the effect! As the English geophysicist Harold Jeffreys remarks, the great difficulty is "to explain the fact that the solar system exists at all," and that "the great trouble is to find even a single solution with any degree of probability whatever."

That an event happening some 2 billion years ago, as indicated by analysis of certain radioactive minerals in the geologically ancient rocks, should be well-nigh impossible to trace in detail is the only comprehensible thing about the problem. With the rapid modern increase in scientific knowledge it is not surprising that in time theories prove more or less unsatisfactory, and that new ones are put forth. The most recent of these theories, announced early in 1948, gives promise of possible success and of certain vigorous discussion. This is the "Dust Cloud Theory" and is sponsored by Fred Whipple, astronomer at the Harvard Observatory.

THE DUST CLOUD THEORY OF WHIPPLE—1948

Ever since the photographs and the arguments of the late E. E. Barnard, of the Yerkes Observatory, proved the presence of vast quantities of opaque obscuring matter in the Milky Way, dust has been a major problem with which the astronomer must wrestle. For example, earlier estimates of distances that were based on the falling off of light coming from stars of known intrinsic brightness had to be reduced when it was found that a good part of the dimness resulted, not from distance, but from the passage of the light through dust clouds. This has been notably the case where the line of sight led through the Milky Way. Indeed, the original measurements of the total width of our disk-shaped stellar system have been reduced from about 300,000 light-years to 100,000 or less.

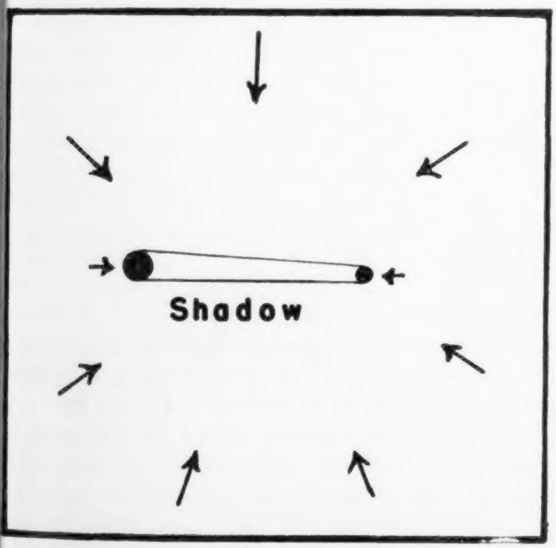
The Dutch astronomer J. H. Oort, who developed the method of measuring the rotation of our stellar galaxy, has calculated the amount of interstellar matter, both dust and gas, to be as great as the total quantity of material in the stars themselves. A part of the gas, and doubtless all the solid matter, are already put together as compounds. What could be better building material for stars and planets?

But aside from such a priori logic there are some observational evidence and calculations that make Whipple's dust cloud theory seem highly probable. Moreover, though there has been too little time since it was first proposed to have all its implications followed out to definite conclusions, this dust cloud theory seems to be freer from questionable features than earlier theories.

The important principle in this theory, on which to base an explanation of the formation of stars and planets from a dark nebula, is that light exerts a pressure against whatever it strikes. The stellar system is full of crisscrossing starlight radiated in all directions from the estimated 100 billion or so stars of all degrees of brightness, some of them more brilliant, some less so, than the sun.

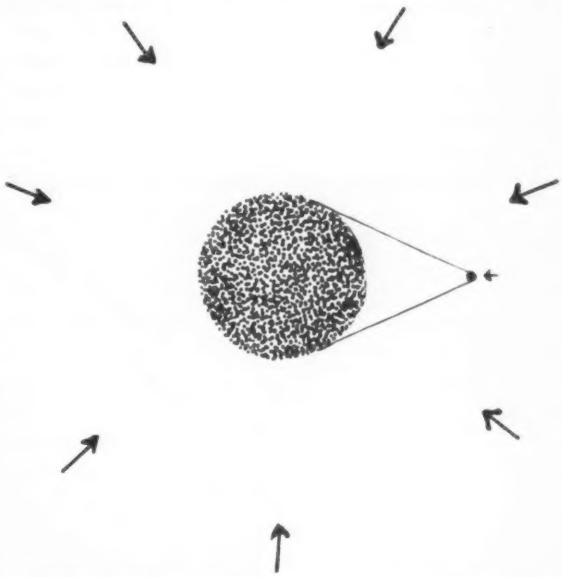
To Lyman Spitzer, of Princeton, is attributed the suggestion that light pressure could be the force that collects dust particles into cosmic clouds. This was a very important step in the theory that pressure of light may have been the initiating force by which particles of matter were driven together to form not only clouds, but stars and planets as well.

Radiation pressure of the sun upon the earth is less than 0.5 milligram per square yard. But this amounts to 2.6 pounds per square mile, and to 65,000 tons against the whole side of the earth facing the sun. This could cause no measurable effect upon the motion of the earth tending to drive it farther away from the sun.



Suppose starlight pressure to be equal from all directions acting on a particle alone in space. Two particles close together would be protected on one side by each other's shadow and so driven together by unequal pressures. The mutual shadow is analogous to a partial vacuum surrounded by air pressure.

Although light pressure is so slight that its effect is unnoticeable upon large objects, it is apparently this force that acts strongly enough upon the fine particles of which a comet's tail is made so that, after the material has once been ejected from the head, the tail points away from the sun (no matter whether the comet is moving toward the sun or away from it), as if a wind from the sun were blowing the light material. Light would naturally have an effect like this on the dust of a nebula among the stars. From indications of their light-scattering effect, the particles of a cloud are estimated to average about 1/50,000 inch in diameter. But because of the greater distance of the stars from the clouds the effect of any one star would be less than that of sunshine on the relatively close comet.



The larger of two particles is less protected from light pressure than the smaller particle. Small particles in a nebula are therefore engulfed by larger ones.

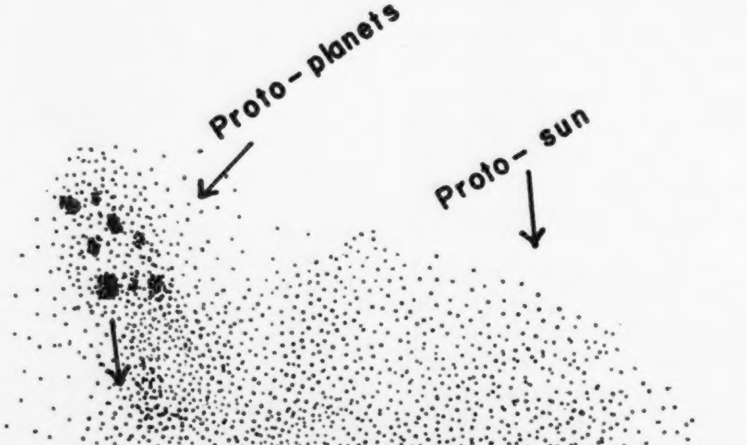
How can light pressure coming from all directions bring even two adjacent particles closer together, to say nothing of helping to build dust clouds, stars, and planets out of such particles? We must remember that, if light falls on one side of a particle from a star in any certain direction, then on the other side of the particle there will be its shadow, so far as that star is concerned. Another particle near by in line with the shadow may be held in balance in all other directions by light coming from other stars, but there will be no pressure on this particle coming from the star behind the one that casts the shadow, for that star's light does not reach the shadowed particle. Hence the particle will yield to this unequal pressure and will tend

to move into the shadow. Indeed, *any* two particles close together will each have a shadow thrown out toward the other star; they will each cut off light coming from opposite directions. The particles will therefore be *mutually* impelled toward each other.

In time the two particles should coalesce and become one. Their shadow, being larger than the shadow of a single particle, will have more power of drawing to it other surrounding particles, and so in time a larger and larger nucleus will be built up. As yet no detailed theoretical study has been made of this final phase of the nebula's collapse into sun and planets.

The apparent attractive force of a shadow is the same as the attractive force that a vacuum seems to exert. When a vacuum is created everything around rushes into it. The explanation is similar, also. Pressure being removed from one side allows the air pressure or light pressure on the other side to exert itself in that direction. The vacuum and the shadow tend to become filled. Thus, light pressure from all directions tends to bring scattered particles closer together and in time form a denser and denser and larger and larger dust cloud.

The next important step in the changing of such a developing cloud into a star or a planet would be when the mass forced together by light pressure became so great and so dense that its gravi-



In a nebula having little or no rotary motion about its center, gravitational forces may yet develop individual or group motions in many directions. The largest group of particles (the *Proto-planets*), having a common and dominant motion, would grow as it swept up lesser groups of different motion.

tational attraction toward its center would become stronger than the force of light pressure around the cloud. Then the outer part would be left behind as gravitation made the inner portion shrink away from it toward the common center. Assuming that we start with a dust cloud having the mass of our sun, it can be calculated that this gravitational pull would become greater than the outer pressure of starlight when the size of the cloud had become about 1,500 times the size of our present solar system.

At this point in the process of the nebula's contraction we must begin to think of the possibility of some of the outer part condensing into a system of planets. Planets could grow from light pressure as the central sun did. So far there is no very decided concentration of matter at the center. Not much heat has been developed; the cloud as a whole has little if any rotary motion; neither is it homogeneous either in density or in what motion there is of the various parts. Here and there in the outer portions the material has condensed into many fairly dense clusters of particles. These condensations are small as compared with the great central mass, and they move about in different directions.

In the course of time these minor clusters cancel out each other's motion by collision or close passage and are gradually drawn into the central mass, the "proto-sun." But suppose one large and rapidly moving group of partially condensed bodies, the "proto-planets," escaped destruction. It would continue to circle around the central mass, all its parts going in the same direction. Gradually these parts, the future planets, would spiral inward, more or less depending upon how much interference they encountered. This interference would put them at different distances from the central sun and would give them more and more nearly circular orbits, since friction tends to change elongated orbits into circles. The planetary orbits now are nearly circular. Likewise, the orbits are nearly in the same plane, a result that can be expected from the fact that the planets are all descendants of one parent cluster of particles with a common dominant motion. Also, the inner side of each planet, the side toward the sun, would meet with more resistance, since the amount of stray material remaining would increase toward the center and hence more of this additional material would be swept up by the inner side of the planet. This would tend to give the planets a direction of rotation similar to their direction of revolution around the sun.

During the comparatively short time of a few hundred years, when the sun was reduced from about the size of the solar system to near its present size, great heat would have been developed by compression of the gases, always mingled with the solid matter of the nebula, and the gases made by the vaporization of all the solid dust particles as the temperature increased. In the last stages of collapse the sun's temperature would rise to thousands of degrees at the surface and to millions at the center. This intense heat would set off atomic changes that would have enabled the sun to maintain its temperature and rate of radiation by means of atomic energy without further help from compression. Masses of planets were too small to have reached temperatures that would release atomic energy.

During the final few months or few years of the collapsing period, while the fiery atmosphere of the sun still extended out far enough to engulf the inner planets, the satellites of Mercury and of Venus, if they had satellites, would have boiled away, together with much of or all their atmospheres, and probably much of the mass of the planets themselves. The outer planets, as far out as can be observed, are well supplied with satellites, and most of them have vast quantities of atmosphere.

The earth, probably, did not escape this "bath of fire" and was hot, perhaps molten. It is relatively devoid of hydrogen and helium and the lighter compounds of hydrogen, so abundant on Jupiter and Saturn.

An observational evidence pointing toward the probable correctness of the dust cloud theory was noticed by Bart J. Bok, astronomer at the Harvard Observatory. He called attention to the fact that there are many small, round, dark clouds among the vast, widely extended nebulae of the Milky Way. Upon measuring the angular sizes of these and estimating their density, Bok found that their masses are quite comparable with the masses of stars. These may be stars in the making. They had often been noticed before, but no significance had been attached to them. They fit well into the dust cloud theory.

An advantage that a nebular theory has over a collision theory is that it goes back a step further into the past. It attempts to account for the sun as well as for the planets. But, in any case, whether we begin our theory after the sun had somewhat the form it now has or whether we go back to the parent nebula, we are still far from the very origin. Even a nebula is a more or less highly organized entity itself. It is made of dust and gas, and these in turn are made of molecules, or at least atoms.

And even an atom is a highly organized microcosm, consisting of matter and energy.

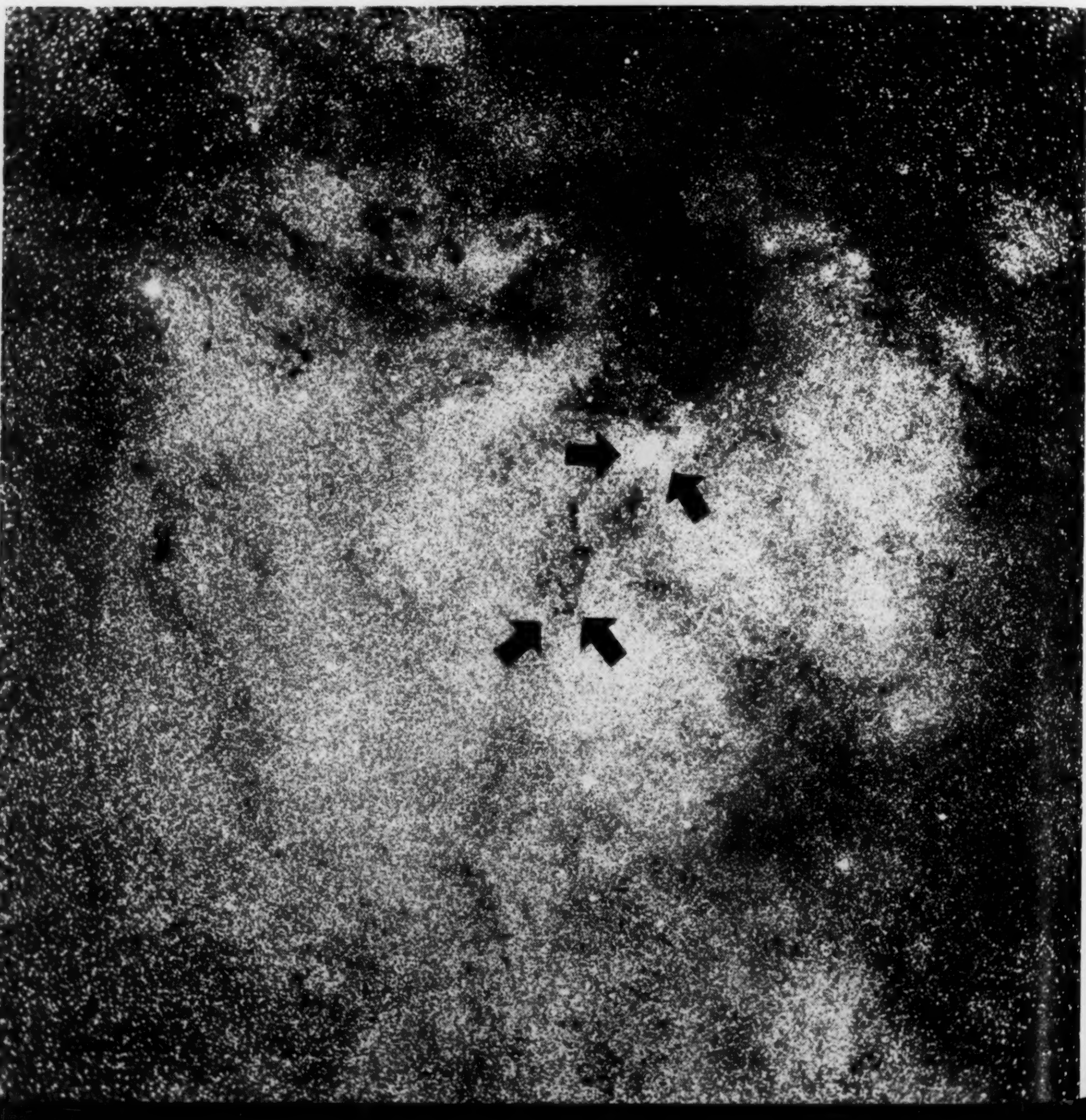
Even after an atom is stripped of all its electrons, the central nucleus, still the same chemical element that it was, possesses properties that so far have defied explanation. What is the tremendous force by which the protons and neutrons of the nucleus are held together? One important step has been taken in man's conquest of the nucleus: its parts can be separated or added to, to form other elements, as was first done in 1919 by Sir Ernest Rutherford when he changed a few atoms of nitrogen into oxygen, and later in a larger way by Ernest Lawrence, and finally on a commercial scale when at Chicago on December 2, 1942, a chain reaction was started in a graphite pile by a group of atomic-bomb experimenters.

The strange relationship between matter and energy has been traced back far enough to find that electrons, which seem to be particles of matter, behave like energy, which travels in waves. Einstein's famous equation, $E = Mc^2$, even suggests the identity of matter with energy.

However far toward origins the scientist may go, he can still see that he has not yet reached the very beginnings. If now or in some future time he may have accounted to the limit of his knowledge for the stars and planets, the atoms and electrons, he will still be in the realm of the finite. From there he can but look, with Lowell, toward the Infinite, where

. . . behind the dim unknown,
Standeth God within the shadow, keeping watch above his own.

Star Cloud in Scutum Sobieski. Contains the cluster M11. Arrows at top show globular cluster, those at bottom, dark circular nebulae. (Yerkes Observatory photograph.)



THE EXPLOITATION OF MINERAL RESOURCES*

T. S. LOVERING

Dr. Lovering (Ph.D., Minnesota, 1924) was professor of economic geology at the University of Michigan for many years and is now staff research geologist of the U. S. Geological Survey. This article was prepared as one of a series of papers on "The World's Natural Resources" presented at the AAAS Centennial Celebration in Washington, D. C., September 13–17, 1948.

THE search for minerals and the exploitation of rich deposits have contributed, and will continue to contribute, much toward directing the course of world powers. It is my hope that if we understand the social and political influence of mineral resources now, we may avoid the disastrous consequences of mineral depletion suffered by many once-powerful nations. The efficiency and adaptability of an industrial civilization are clearly dependent on minerals as well as on men. In a sense minerals are the food of industry: iron and fuel are its chief sources of strength and energy; but aluminum, copper, lead, and zinc are the "bone builders" essential to its growth, and manganese, chromium, nickel, molybdenum, tungsten, and many minor elements are metal vitamins absolutely essential to its health. A few countries, including both the Soviet Union and the United States, have an abundance of iron and coal, but none has a satisfactory supply of all the essentials to the well-balanced diet. All suffer from a deficiency in some of the metal vitamins; the United States is quite dependent on foreign sources for manganese, chromium, nickel, tin, and several others.

Mineral deposits have many unique characteristics. Their seemingly haphazard position is fixed by some geologic accident of the remote past and not by our convenience; they are finite, nonrenewable resources, and once taken from the ground there is no second crop; during exploitation the unit cost of production rises, especially as a deposit nears exhaustion; continuing production requires discovery of new deposits—or extensions of known bodies, year after year; and, finally, most mineral products are either expended in their first use, as with coal and oil, or else last for decades—even centuries—and may be reused over and over, as are iron and gold. The nonexpendables accumulate through the years in reservoirs of potential scrap.

As some mineral deposits are found only in sedimentary rocks and others only in close association with igneous bodies, the rock formations exposed in a country suggest its probable mineral resources. Moreover, sedimentary deposits of the same age commonly contain similar deposits over wide areas, as witness the coals of the Carboniferous period in North America and Europe, and the remarkably similar sedimentary iron ores of pre-Cambrian time in the Lake Superior region, northern India, eastern Brazil, and central Labrador.

Ores related to igneous rocks show far less tendency to age correlation than do the sedimentary deposits; instead of a time relation, a geographic localization is apparent, and many well-defined mineral provinces have been recognized in which the ores affiliated with igneous rocks are characterized by a certain suite of metals and the absence of others. In Bolivia tin is a common metal in the mineralized areas associated with quartz porphyries of Tertiary age; a large number of mineral districts in the United States are also associated with quartz porphyries of that age, but tin ore is completely lacking.

The special conditions of environment and geography that cause peat, or salt, or bog iron beds to form today are restricted to small areas irregularly scattered about the globe; so, in general, have they been in the geologic past. The haphazard position of present-day volcanoes in so-called volcanic belts suggests the unsystematic and restricted occurrence of ores related to ancient igneous bodies. The spotty distribution of mineral deposits is most unfortunate for a disunited world; even such comparatively well-distributed sedimentary formations as coal have a very erratic occurrence on our planet; South America, Africa, northern and southern Europe, and much of Asia contain almost no first-rate black coal; in contrast, about half the world's reserves are concentrated in a small part of the United States. Mineral deposits genetically related to igneous rocks are, as a rule, even more localized. Some striking examples of this group are provided by nickel, molybdenum, and quartz crystal. Nearly all the world's supply of nickel

*Published by permission of the Director, U. S. Geological Survey.

comes from a single district in Canada; for many years a mine at Climax, Colorado, has produced most of the world's molybdenum; and Brazil supplies virtually all the quartz crystal used.

Examples of this sort could be multiplied many times. The inequality in the distribution of high-grade deposits of the different metals and minerals is tremendous and has profound economic, political, and social consequences. Civilization and culture do not remain static; they advance or retrogress. The exploitation of easily recovered mineral resources has commonly coincided with a period of national power; their depletion with national decline. Nothing should be clearer to a scientist, however, than the fallacy of the belief that "history repeats itself;" history is full of similarities and parallels, but not of repetitions.

Although history does not repeat itself, a pattern may be seen in it that is well worth thought, if we wish to use history as a guide. I shall call attention very briefly to a few of many illustrations of the influence of mineral wealth on the rise and fall of nations. Human activity has been determined by the accident of mineral occurrence far more than most historians recognize. Flint was sought for the hunter, clay for the potter, metal for the wealthy, even at history's dawn. Later, as tribes merged and nations evolved, liquidation of those quick assets of a country, its mineral wealth, yielded revenue that many times led to power. But commonly we fix our attention on the power, not on the ores in which it had its roots. The success of Alexander the Great came not alone from his genius, but from the well-equipped army with which he started his campaigns—equipment and men available because of the many thousands of talents of gold accruing to him and to his father from the Macedonian gold bonanzas. Some thirteen centuries later, the long-lived Rammelsberg silver mines, discovered about A.D. 920, were a vital factor in the birth of the Holy Roman Empire; bullion from these mines provided both Henry the Fowler and Otto the Great with much of the capital used in their successful campaigns. Mineral deposits also contributed immeasurably to the expansion and power of ancient Egypt, the success of the exploring Phoenicians, the civic and military vigor of ancient Athens, the rise and fall of Venice, and the brief hegemony of Spain.

The influence of metals since the industrial revolution has been all too evident. It is not a mere coincidence that Great Britain was the most powerful nation in the world during the nineteenth century, when she led the world in mineral production. During this time she reached the peak of domestic mining, followed by the inevitable period of declining output, during which she acquired foreign properties and increasingly depended upon imports of ores to run her industrial plant. For a century and a half prior to 1850 Great Britain produced over half the world's supply of lead, and from 1820 to 1840 furnished almost half its copper. As the island kingdom approached the zenith of its power in the later half of the century its iron production increased from one third to one half the world's total. The peak of domestic production of lead was reached in 1856, of copper in 1863, of tin in 1871, of iron ore in 1882, and of coal in 1913.

The United States has passed the peak in domestic mining for only a few metals, but we can say with confidence that the high point was reached for mercury in 1877, and for lead in 1925; it is quite probable that the peak for some other metals was passed in 1943. Acquisition of foreign sources of supply by American capital is proceeding rapidly, paralleling British moves a few generations earlier. The cost of delivery at United States industrial plants, however, will be substantially more than in the era of lush production from domestic sources of supply.

The Soviet Union has mineral resources of major importance and was late in starting large-scale exploitation. It is unlikely that the Soviet Union has yet achieved peak production in any of its important resources. Its rapid industrialization and fast-growing population provide internal markets demanding constant expansion of mineral production—or procurement. Like other industrial nations, it has an ever-growing concern for the position, extent, and availability of mineral resources at home and abroad.

The scattered world sources of all the many minerals required by industry need not be enumerated, but the adequacy of the total known supplies is a matter of deep interest to those aware of the profound social effects implicit in the exploitation of mineral deposits today.

Estimates of reserves of currently and potentially usable ore in the United States were made by the Geological Survey and the Bureau of Mines during the war and are now available in the book *Mineral Resources of the United States*. Mr. McKelvey, of the Geological Survey, recently called my attention to the fact that many of these estimates are roughly proportional to the average concentration of the various metals in the earth's crust. If we multiply the average percentage of any element in the accessible lithosphere by a billion, the result approximates the tonnage of the known reserves of

our more plentiful ores in the United States. It is interesting to note the general relation between these figures, the official estimates of reserves, and our concern for supplies. About 4 percent of the continental crust of the earth is iron; our known reserves are about 2 billion tons of iron in ore, and several times this amount in potential ore. Although some iron is imported, there is little official concern with our iron ore supply. Copper, of which we often have a small exportable surplus, makes up 0.01 percent of the earth's crust; a billion times 0.01 is 10,000,000, and our reserves in this metal are about twice this figure. Cadmium is present to the extent of only about 0.00005 percent, and this figure times a billion tons gives us 50,000 tons, just equal to the estimated reserves of this rare metal. The United States produces her needs in cadmium and at the present rate of consumption has fifteen years' supply in sight.

Turning to those metals for which our reserves are less than the amount calculated by this empirical method, we find our mineral deficiencies accented. Our reserves of manganese are about 7 percent of the calculated amount, of nickel 1 percent, of tungsten 0.4 percent, of tin 3 percent, and of platinum 2 percent; all these would be classed as strategic metals for the United States in the event of another war.

We have no adequate published estimates of world reserves of most metals. For a rough approximation, however, the figure of ten billion times the average percentage of the elements in the earth's crust suggests the world's commercial reserves, except those like coal and phosphate rock that are primarily the result of long-continued biologic processes of concentration. World reserves of such resources may be larger by one to three orders of magnitude.

As the grade of material designated "ore" is dropped, the reserves increase, but the cost of the metal won from the lower grade is also larger. This added cost is a measure of the additional energy—mechanical, chemical, mental, and physical—required to get the desired product from the lower-grade deposits. This cost means a lower standard of living than that possible with cheaper raw materials.

At the United States level of consumption for 1945, our coal, phosphate rock, iron ore, and molybdenum ore seem adequate to last for several generations, with only slightly lowered grade; if we depended solely on our known domestic resources and used current technology, our copper, aluminum, zinc, gold, and possibly our petroleum might last for twenty years; but manganese, vanadium, and lead would be used up in from one to ten years; platinum, antimony, mercury, tungsten, chromium, nickel, tin, mica, graphite, asbestos, diamonds, and quartz crystal are present in such small quantities as to be negligible.

All industrial countries should expect their domestic ores to provide gradually diminishing and more expensive raw materials and should expect to supplement them with increasing imports of foreign ores and metals. Concern over adequate dependable sources of supply will become more and more evident in national policies as domestic raw materials become scarce. Stock piles may be built up for emergencies, but they are essentially a military expedient and in no way take the place of cheap domestic ores. The sooner we can dispense with them and depend on normal economic foreign trade, the better it will be for all. Unfortunately, much of the scarce supply of "strategic minerals" now finds its chief market in armaments, where metals are withdrawn from constructive use; they will continue to make this negative contribution to society until the peoples of the world achieve the mental disarmament which alone can do away with the occasion for all war.

It is known that world reserves for mineral materials also range from some that are adequate for thousands of years to others that may be used up in one or two decades. From the long-range view the world is in short supply of zinc, lead, tin, mercury, platinum, petroleum, quartz crystal, mica, and industrial diamonds.

We in the United States would like to see the standard of living of all people raised to that which we enjoy. It is interesting to note that we make up about 6 percent of the world's population and, at a conservative estimate, we use about 40 percent of the world's output of all mineral products. Although part of this is exported, our unsatisfied internal market could easily absorb the export equivalent. If China, India, and the rest of the world were supplied on the same basis, the world output would have to be increased about 700 percent, and the reserve of the grade currently used would seem alarmingly small.

The world's reserves appear sufficient for many years under the present economic conditions, but it could happen that our grandchildren will be hard-pressed to find economic supplies of many of the industrial minerals of today. If we can foresee a time of increasing social and economic strain, ten, fifty, or even a hundred years in the future, that could be eased or eliminated by starting work on a problem today, we must act or our civilization will fail of its opportunity to raise man to his poten-

tial stature. Toynbee likens civilization to men struggling from ledge to ledge up a precipice; using this simile, we can say that the United States stands with momentarily secure footing and may with little exertion boost civilization well along toward its unseen goal.

Strategic minerals and metals are commonly defined as those for which we have no substitutes, which are essential to industry, but which cannot be produced at home in quantity sufficient to satisfy industrial demand. It is true that most of them have no satisfactory substitutes at present, but when we say a material is essential to industry and that it has no substitute, two unstated qualifications are often forgotten: a given material may be essential in producing the most desirable product, or may be essential to successful commercial competition in world markets. Belligerent insistence on completely adequate supplies of strategic materials at a "fair" price from a foreign country has proved expensive for several nations in the past. It will usually be profitable to pay more or to accept somewhat less satisfactory substitutes and forego commercial success in competition for restricted markets. The discovery of satisfactory substitutes for any material in short supply is a major contribution to both national economy and world harmony.

Where great disparity exists in the cost, efficiency, or utility of products fabricated from readily available substitutes, a dual effort should be made to acquire and conserve the strategic material for the essential uses and at the same time to find better substitutes. The wholesale use in jewelry of world-scarce platinum, one of our most useful irreplaceable research metals, seems short-sighted. The use of helium, gold, uranium, and thorium have all come under strict government control in the United States within our generation. This trend will probably continue both here and abroad; profligate use of strategic minerals should be curbed by wise regulatory restrictions.

If the industrial nations are to avoid conflict over the scattered major foreign sources of cheap raw materials, everything possible must be done to lessen the need for those strategic minerals that now seem vital to their economy and that will probably be in continuing and increasingly short supply. At the same time each industrial nation should husband its known resources and search diligently, intelligently, and hopefully for new supplies.

The United States is now the richest and most powerful nation in the world, as Great Britain was in the days of our grandfathers. I see no unavoidable calamitous decline in store for us, but I think we all realize the probability of trouble ahead if no preparation for lean decades is made during the fat and prosperous years of the present. We can well afford to spend much on research that prepares for future needs now easily foreseen. We cannot afford to do otherwise.

Geological research and metallurgical research on low-grade, currently noncommercial sources should anticipate the demands of the future. The land, the sea, and the air will all richly repay concentrated study; in them almost inexhaustible reserves of certain elements are assured. Not only should we learn to mine and concentrate low-grade materials in the most efficient way, we should also devise the most economic metallurgical techniques of extracting the contained metal, oil, or other compounds sought. The infant science of alloys—of foretelling the physical characteristics of combinations of elements—should grow into one of our most useful aids if adequately developed. It is a conservation policy of obvious importance to seek substitutes among the abundant elements for our deficient special-use metals.

New deposits continue to be found from time to time, and discovery of a single rich body of one of our essential minor metals could assure an abundant world supply for generations. Unfortunately, however, the rate of discovery of mineral deposits has been decreasing for many years. The fundamental research that should lead to discovery of the more securely hidden ore bodies has moved forward at the pace of a sleepy snail. Tremendous opportunities for progress exist in the borderland fields where physics, chemistry, and geology overlap, but hardly a handful of men are at work on the many critical problems that must be solved there before the art of ore-finding moves toward a scientific technique. Nor is the oil geologist much in advance of his admittedly backward brother, the mining geologist. A major cause of our slow progress in the past has been the difficulty of finding research scientists adequately trained in physics or chemistry *and* geology.

It is possible, however, to accomplish amazing results when groups of men specializing in different disciplines coordinate their efforts. Such group research needs subsidies and should have both short- and long-range objectives. It often yields, however, almost unbelievable results in a comparatively short time, as witness the many militarily successful developments of the war years and since. The most spectacular group research has been done in gaining understanding of the room temperature, one atmosphere microcosm, and in devising engineering applications of this knowledge. In contrast,

much laboratory work at high pressures and elevated temperatures will have to be done before we understand the origin and localization of oil and ore deposits. We know little more now than a century ago about the source of ores, the chemistry of their formation, the reasons for their concentration in ore bodies, their relation to several associated barren phases of mineralization—usually lumped together and dismissed as "alteration"—or that amazing phenomenon called replacement, a chemically unreasonable process that changes the substance but preserves faithfully the original texture, form, and volume apparently without benefit of stoichiometric relations. Obviously our success in finding additional supplies would be greatly enhanced if modern systematic research were directed toward elucidation of the genesis of metallic ores—comparable with the great work done by the Geophysical Laboratory of the Carnegie Institution on the origin of igneous rocks. Similarly we could make great strides in preparing for the future by laboratory work on the genesis of vital nonmetallic minerals. Such investigations, together with expansion of the current studies on synthetic crystals that are yielding such instructive and practical results, promise eventually to relieve some of our most severe special mineral shortages. The techniques are now available that should lead to commercial synthetic mica and commercial radio-grade quartz crystal. Ultimately, the more difficult problems of synthetic asbestos, diamonds, and other important industrial minerals should be solved.

Present progress in the purely geologic aspects of oil, ore, and other mineral deposits is much more satisfactory. Empirical knowledge of the relation of mineral deposits to geologic structure, to the chemical and physical make-up of associated rocks, and to the geological environment increases steadily. This is at present our chief hope of improving the discovery rates. I believe that the field man has already made a good start on the hard task of finding ore hidden well below the surface.

The use of sensitive chemical methods for finding soil-covered ore bodies or detecting minute additions of ore minerals in barren rock over blind ore bodies promises to be a fruitful area of geochemical research. This field owes much to the excellent pioneer work done in it by Scandinavian and Soviet scientists.

Geophysical prospecting—geologic interpretations based on the use of instruments to determine physical properties of rocks in place and their response to various fields of force—has been much more successful in helping the geologist find oil than ore. The comparatively simple structure of most oil fields contrasts with the complexly broken inhomogeneous terrane where so many of the nonsedimentary mineral deposits are hidden, and geophysics is inherently better adapted to unraveling the simple than the complex. High costs and less than fair success in getting ore have resulted in the generally conservative attitude of mining companies toward geophysical work, but I expect geophysics to play an increasingly useful—though limited—role in future exploration for ore. It will continue to contribute much to the discovery of new oil fields and should be used more and more in the appraisal of ground water, a valuable mineral that is fast being depleted in many areas.

Earnest, thoughtful research, carried on in the field by the geologist, chemist, and physicist, checked constantly against laboratory work, will suggest new methods of prospecting. There should be at least one adequately financed group devoting full time year after year to the job of devising, testing, and making available new techniques without the disheartening insecurity that accompanies uncertain appropriations, or the unsettling stimulus of possible financial jackpots for each new idea. The nation and the world community need fewer patents and less secrecy, and a little more interest in the unselfish solution of their common problems. As new methods of prospecting are devised they should be field-tested—with drill or excavation. The essence of the scientific method lies in rigorously testing new theories; progress is difficult when theories are plausible but neither disproved nor established.

In addition to scientific research and to technological progress, we desperately need wise planning for economic and equitable utilization of the world's widely scattered deposits. At the present time, however, much of the basic data required for such planning are missing. Our knowledge of the world's reserves is of the crudest sort. Inventories of our own deposits are admittedly inaccurate, but far better than those that can be obtained from most other countries for years to come. Geologic work, exploration, and ore estimates should be greatly accelerated so that an inventory of our planet's resources will be at hand soon—in time for the next generation to use in planning the make-up of those global economic units that will most effectively utilize our natural resources to the maximum benefit of all mankind. We are well warned that we must plan for tomorrow if catastrophe is not to ride us down, but, with confidence that we shall solve tomorrow's problems, I suggest that it is none too soon to give thought to the years to come after tomorrow.

THE PHANTOM WATERFALL

(THE DRY FALLS OF THE COLUMBIA RIVER, GRAND COULEE, WASHINGTON)

BARBARA WHITNEY

They say there is seen in the moonlight
And heard when the night is still
A cataract's mighty thunder
And the mist where the waters spill—
Raging and foaming in anger,
Hurtling down from the wall
They have carved in torrential flowing—
The Phantom Waterfall!

Through the towering cliffs of Grand Coulee,
Blocked by the glacial field,
The Columbia turned in her coursing,
And the torrent plunged and reeled . . .
In the script of the prehistoric
She wrote on the lava there
The romance of an old, old river,
Cascading her long white hair . . .
Mightier than Niagara,
She carved, through the myriad years,
The gigantic rocks of the canyon
Where fell her giant tears . . .
And lo! When the glacier melted
And skies were warm again
She turned back to her ancient channel
To flow on in the sun and the rain . . .
And the horseshoe falls of her making,
She left them mute and dry,
A testament to her story
For the retrospective eye . . .
For the mind of man that is probing
The long, long ages past . . .
And the heart of man discerning
Eternity's circle, at last
Closing upon its beginnings—
Chaotic and misted and gone—
Night into morning and noontide,
And evening and night into dawn . . .

And they say that sometimes in the moonlight,
Sometimes when the night is still,
You can hear the voice of the Phantom,
Where the white ghost waters spill!

ATTITUDE MEASUREMENT AND THE QUESTIONNAIRE SURVEY

ARNOLD M. ROSE

Before joining the Department of Sociology and Anthropology at Washington University Dr. Rose was associate director of the Carnegie-Myrdal study of the Negro (the results of which were published in American Dilemma*) and project director for the War Department's Research Branch. His article points out some of the weaknesses of commercial polls, so strikingly demonstrated in the 1948 Presidential election.*

MUCH of the basic raw material of social science consists of the beliefs and attitudes of men. These presumably exist in men's minds, and one task of the social scientists is to get a fair representation of them down on paper. Traditionally, this work of the social psychologist and the sociologist has been known as "attitude measurement," although recent commercial use of some of the techniques has popularized the term "public opinion poll."

To some outsiders, the public opinion poll looks deceptively easy: you just ask people questions, add up their answers, and you know what they are thinking. To other outsiders, a correct representation of men's thoughts is an impossibility: how can you ever know what people are really thinking simply by asking them a few questions, when it is so easy to dissemble and when even a psychoanalytic probing of two years' duration sometimes fails to get beyond all the conscious or unconscious defenses? Three decades of research and testing give the lie to both types of critics: it takes many technical devices and a good deal of skill to measure attitudes with validity and reliability, yet we know that it can be done. The early doubt that you could "really" get at attitudes through questionnaires was justifiably based upon a lack of concern on the part of opinion measurers for anything but a check mark or a verbal statement. As attitude measurers became more interested in the meaning of their raw data, this criticism became less valid. This must not be taken to mean that every published study of attitudes or every newspaper poll should be taken at face value. There are still numerous charlatans in the field, both with and without college degrees, who find it either profitable or prestigious to publish unsound figures. This has been especially true since reports on public opinion have become salable goods—to businessmen, to special-interest groups, or to the general public through newspapers and magazines.

MAJOR REQUIREMENTS

Let us briefly review the major requirements of a sound public opinion poll. In the first place, the questions must be capable of being answered adequately. There is little purpose in asking people questions for which they do not have an answer, or for which they cannot readily formulate an honest and complete answer. To rely on a refusal to answer is not sufficient, since some people seek to avoid the appearance of stupidity or ignorance by giving an answer when they really have none, and other people hesitate to appear dogmatic or overcertain by stating their true attitudes, and so say that they "don't know." Words serve as a vehicle for the question, and most words have at least a bit of ambiguity. It is essential that the words have a minimum of ambiguity as well as that they be simple and understandable to persons of little education. Even the person who has had long experience with formulating questions will occasionally be amazed when he poses a question, which he thinks is straightforward and unambiguous, to a variety of people and finds several variant interpretations. American culture is so far from being homogeneous that even the meanings of words shift markedly from one group to another. The skilled attitude measurer is aware of these cultural variations in so far as they apply to the more common words. Yet no reputable investigator will permit a questionnaire to go into the field without having "pretested" it on a range of individuals, representing the principal social groups, and experimenting on word selection with them.

There are other problems connected with questionnaire formation. Even when the question is completely and correctly understood, its specific wording can influence the direction of the answer in most instances. It is only when people have thought through all the ramifications of an issue, and have reconciled their conflicting motivations toward it, that they will not be influenced by the

subtle connotations of the words constituting the question. Most attitude measurers have sought to meet this problem by choosing only "neutral" words and in other ways seeking to avoid the "biasing" of a question. Other investigators feel that no question is unbiased in an ongoing social situation and therefore seek to probe an attitude with a battery of five or six questions, some biased one way and others another way. This has led to the development of the "attitude scale" (which we shall consider later). The recognition that answers to biased questions may be significant also led to experimentation with questions that indirectly reveal attitudes of which the respondent may be completely unaware. Life itself is full of suggestive influences, and it may be as important to know whether an individual will respond to a subtle suggestion as it is to know how he will respond to a straightforward unbiased question.

The complexity of human motives has been exploited in questionnaire formation in other ways. Numerous studies have shown that the order of questions in a questionnaire will influence the response, and investigators must not only be wary of this, but must adjust for it by varying the question order in any one study. The content, or subject matter, of the question is also important. Kornhauser has demonstrated, for example, that many studies of attitudes on labor issues used a content that sought only to get at negative attitudes toward organized labor. If, instead of asking questions about strikes and monopolistic practices, the studies posed questions about security and bargaining equality, the conclusions regarding the status of unions in American public opinion would have been quite different.

After a questionnaire is formulated, a second major set of problems arises. How, and under what circumstances, shall the interview be conducted? There are innumerable empirical "rules" on how to establish a good relationship with an interviewee, varying from an admonition against interviewing anyone where a third person can hear the answers to an injunction against expressing the interviewer's own attitude either by word or subtle gesture. Some critics who have done little or no interviewing themselves question whether people will truthfully answer questions, especially when the questions become intimate and personal. Those who have experience in interviewing know that it is possible to question successfully about any subject if the interviewee is "approached in the right way." With very few exceptions, people seem to like to talk to strangers, especially if they feel their answers have some value, if they understand the purpose of the interviewer, and if they are convinced that they will be strictly anonymous. The interviewer must be sympathetic toward the interviewee so as not to inhibit him from expressing an unusual attitude, but nevertheless must maintain his social distance, since it is the impersonality of the interview situation that seems to bring out full and frank answers. Studies have been made to check the validity and completeness of answers secured by good interviewers when interviewing on very touchy and personal subjects, and it is startling to see how correct the information is.

The problems of interviewing remain for most investigators, however, because of the difficulty in securing or training good interviewers. Checks have had to be devised to curb interviewer cheating. A disturbing finding was that interviewers drawn from low-income levels secured answers different from those brought in by interviewers who had middle- or upper-income backgrounds. Conscientious investigators are now training their interviewers more carefully and are trying to select interviewers with backgrounds representative of the general population.

There is a difference of opinion as to whether interviewers should rigidly follow a fixed schedule of questions or should be free to develop their own questions after a fixed first question sets the subject for discussion. The fixed schedule reduces interviewer bias and the need for high interviewing skill. It also has the advantage of permitting easy classification and tabulation of the answers for quick reading. The flexible schedule permits probing for subsurface and complex attitudes. It allows the respondent to state his attitude in more than a phrase, and prevents misunderstanding about the meaning of questions. The resolution of this disagreement about the rigidity or flexibility of questions seems to have taken the form of a general recognition that both forms have greatest value for different problems and different subjects.

A new difficulty has arisen to complicate interviewing in recent years. When people have strong fears, they are too suspicious to be readily interviewed. This is why it has always been impossible to conduct a public opinion poll under a dictatorship. People will not talk freely to an interviewer who might be a threat to them. In the United States, until recently, this was significant only when interviewing Negroes, who were so afraid of expressing their true opinions to a white person that they either "played dumb" or told the white interviewer what they thought he would like to hear. Reliable investigators always met this situation by employing Negro interviewers and clear-

ing their survey with Negro defense organizations or leaders. Since 1946 the same situation has arisen with a significant number of white respondents: they are suspicious of strange interviewers as possible Communists. Interviewers for such nationally known polling agencies as Gallup and Roper can easily identify themselves, but the problem remains for independent social scientists. The only partial solution that has been devised has been to clear studies with churches and civic organizations.

A third important factor in public opinion research is the selection of respondents. Only the Census Bureau has the resources to interview every family in the United States, and then only once in ten years. For all other studies, a sample of respondents must be selected, and the sample must be representative of the population about which the investigator wishes to generalize. The rules for drawing a representative sample have been well worked out by statisticians, but they are difficult to apply when human beings constitute the sample. When physical objects, or even plants or animals, are the subject of investigation, it is a relatively easy task to pick cases at random, making sure that each unit has the same chance of being selected as any other unit. When the people of the United States are the subjects of investigation, however, it is impossible to line them up for purposes of picking a random sample or to obtain equal access to all of them. There is not even a list of their names and addresses from which a sample list could be selected and then traced.

Two types of methods have been contrived to meet the problem of selecting a representative sample of people. One is the "quota" method, by which a sample is devised to match the distribution of the population in certain known traits, such as age, sex, region of country, rural-urban residence, etc. (This information is available from the decennial census, corrected for postcensal changes and for demonstrated errors.) Then interviewers are sent out to interview people who have the specified traits: they fill up the quota of each age-sex-regional, etc. type of person as fast as they happen to come across persons of that type. This method of sampling is widely employed by commercial pollsters and was the one chosen by the one biologist who has elected to do a major social science study—Kinsey, in his study of sex behavior. The method has one grave weakness, however, in that the people *within* each of the quotas may not be representative. This is of no great importance if the biases of nonrepresentativeness within the quotas are not related to the subject matter of the interview. But if aggressive people happen to be chosen more frequently than passive people, it does make a good deal of difference, for example, if one is questioning about attitudes toward war. If working women are not available to a poll-taker because they are working during the daytime, it will make a great deal of difference in an election poll, since working women vote differently than housewives. The careful social scientists have therefore devised another method of sampling, called the "area" method. This involves dividing the population into small geographical areas, selecting some of these areas as representative on the basis of known traits, and using carefully controlled random sampling of people within each of the selected areas. This method is more difficult and more expensive to employ, especially in rural areas where the dispersion of population makes random sampling hard to accomplish. But it seems generally to be more foolproof than the quota method.

The problems of the attitude measurer are not done when he has devised his questionnaire, selected his sample, and secured his interviews. Then he has to boil down his mass of information into a comprehensible and readable form. This involves the steps known as "coding" and "analysis." Coding is essentially a process of classifying the diversity of answers, and the words in which the anwers are couched, into a reasonable number of categories. Actual deviations must not be lost, but meaningless diversity must be submerged. The planning of a code requires skill and knowledge; the application of a code takes a great deal of tedious work. The principal step in analysis is deciding how the coded answers are to be tabulated. Should answers to a given question simply be added up, or can answers to one question make sense only when put in context of answers to another question? Are the facts for the population as a whole what is important, or is significance to be secured only by comparison of answers from different elements in the population? Do answers to a single question have any validity, or is it necessary to combine into a scale the answers to a series of questions all trying to get at the same attitude? These are some of the questions the analyst must answer as he tackles each study. He cannot wait to solve these problems after the interviews are complete, but must have his solution fairly well worked out before he regards his questionnaire as complete and before he collects a single interview.

The interpretation of findings and the presentation of results offer a final set of challenges to the attitude measurer. No uniform standards have as yet been devised to handle this stage of the operations. Nor probably can any ever be, since each

subject of study is somewhat different from every other. A few general principles have been set forth, and a variety of devices for presentation have been examined, but these are too detailed to go into here.

USES OF THE QUESTIONNAIRE SURVEY

What sort of information is the product of all these operations? Although the questionnaire survey grew up in an effort to measure attitudes, it now has a far broader use than that. In addition to detecting attitudes, it can reveal expectations, wishes, activities, facts, estimations, and so on. Let us translate these vague terms into examples of concrete uses to which the questionnaire survey has been put.

One is the simple determination of facts, such as the amount of liquid savings in the hands of the general public, which the U. S. Treasury regularly asks the Survey Research Center at the University of Michigan to determine. Sometimes facts that are easily detected separately have no significance until put in context by a questionnaire survey. Certain Army officers in the Mediterranean Theater were disturbed at the sloppy appearance of American soldiers from the Replacement Depot at Pozzuoli until a survey determined that not only were there no QM laundry facilities available at the Depot, but that there was no soap available at the PX and that the military police did not allow soldiers to take clothing out of this camp for fear they would sell it on the Italian black market. The Kinsey study of the distribution of different types of sex expression is another example of fact determination, where the main interest lies in comparison of different groups in the population.

The preference type of study is most frequently employed by large industrial concerns. A great deal of money is spent every year to determine what various consumers find most pleasing to the eye, to the nose, to the palate. Government has also used the preference survey when it has a choice and wishes the decision to be made in accord with the preferences of the citizenry. In planning its postwar education program, the Army used a sample interview survey as the equivalent of getting a filled-out commercial order. The social scientist seldom has any direct use for a preference survey, except when preferences can be used as indices of attitudes. Many of the early studies of attitudes toward minority groups, for example, involved the respondents' making statements of preference for one group as compared to another, in different social situations. The psychologist L. L. Thurstone developed a complicated technique for transforming such preference statements into a scale of attitudes reflecting the status of minority groups, vocations, and other rankable social traits.

The knowledge type of questionnaire survey is very much like a school examination, except that it is given to the usual representative sample of the population and its purpose is not to rank. Rather, its purpose is to determine whether a publicity campaign of one sort or another is successful in reaching the public or whether those who have greater knowledge about something also have attitudes more favorable to it. Government policymakers—as well as manufacturers—are interested in such information. Social scientists have found use for many studies of the opinion or personality correlates of knowledge. In general, they have found that those with more knowledge about a given subject have fewer emotional prejudices against that subject.

Other types of cross-classification are employed in an effort to get clues to the causation of attitudes and behavior. In one study of venereal disease, not only was there cross-classification of incidence of this disease with knowledge of preventive techniques but also with facts about drunkenness when having extramarital intercourse and with beliefs about the ability of newly discovered drugs to cure the disease. The findings confirmed the hypothesis that little knowledge, drunkenness, and overconfidence in modern drugs were strongly associated with the incidence of venereal disease. Correlations by themselves are insufficient for imputing cause, and so experiments were carried out to test the tentative findings. Information was given about techniques of preventing venereal infection and about the uncertainties of available drugs, with the object of determining whether this resulted in a reduction of the venereal rate.

Most experimental studies in the social sciences require attitude measurement at every phase. The usual procedure has been to administer a questionnaire to matched experimental and control groups. Then the social stimulus is directed at the experimental group, sometimes quickly, but more often over a period of time and without obvious connection to the questionnaires. After this, the same or equivalent questionnaires are administered to the same groups, and changes in the attitudes of the experimental group are compared to changes in the attitudes of the control group, if any. The questionnaire may be repeated after an interval of several months to see whether the effect of the stimulus has worn off. Sometimes several stimuli are directed at different experimental groups at the same time, and a comparison made of their relative influence. Such, for example, was a study of

the relative effectiveness of emotional and logical propaganda. For the particular groups and subject matter chosen, it was found that the emotional appeal had greater success than the rational appeal, but that the latter was more effective than no appeal at all. Some experimental studies are "evaluational" when the stimulus investigated is a program that someone desires to evaluate.

Another type of attitude survey might be called a "definitional" study. Definitions are usually set by a tradition or by operational convenience, but sometimes it is valuable to conduct a little study before formulating a definition. One such study might be to determine whether people generally make the same distinctions between terms or set the same limits to terms that the scientist does. A certain study translated the numerous current definitions of the term "morale" into a questionnaire in order to determine the extent to which high morale in one sense was related to high morale in another sense. Such diversity was discovered that it was necessary to formulate several distinct definitions of morale and to note that they were not the same thing. Some investigators find important social science data in popular definitions, and they conduct surveys where the questions allow the respondent to formulate his own definitions.

A final type of study that must be mentioned has prediction as its primary purpose. One prediction study may simply be an extrapolation of attitude trends. Another may be based on influences from causes discerned from the correlational or experimental types of studies. A workable schedule on such a basis has been prepared by Burgess and Cottrell to predict the future marital adjustment of an engaged couple. A third may be a questionnaire survey in which respondents are asked to state or estimate their future plans. Some valuable clues to the future of the consumers' market have been discovered by asking people their plans for buying radios, automobiles, or household equipment. The election pollsters have this last type of purpose—to predict simply by asking people what they intend to do. But the method is risky if not carried out in conjunction with questions about motivation and intensity of attitude, and if careful analysis is not made of the "undecided" voter.

Sometimes a prediction study can be used as a a selective technique to prevent the predicted events from occurring. Such was a study carried on in the Army during the second world war to predict which newly inducted soldiers would develop psychoneuroses. After it was determined which traits, experiences, and reactions predisposed a man to psychoneurosis, those inductees who had a high score on the predisposition test were sent to a psychiatrist, who either rejected them for Army service or marked them down for further observation.

There is an increasing range of uses for the questionnaire survey. Each time there is a new use or a new criticism, there is development or refinement of techniques. In 1944 Hadley Cantril brought together existing knowledge about techniques in this field in his book *Gauging Public Opinion*. Developments have been so rapid, however, that already many sections of the book are out of date. Even the periodical literature is somewhat behind, as one can observe when he attends the annual meetings of the World Congress for Public Opinion Research, the American Association for Public Opinion Research, the American Statistical Association, the American Sociological Society, or the American Psychological Association. The presentation and discussion of new techniques are carried on under the most favorable conditions of open-mindedness and high interest. Those practitioners with a scientific orientation know their own limitations. The field appears to have a most interesting future.

EARLY HISTORY OF INFRARED SPECTRORADIOMETRY

W. W. COBLENTZ

Dr. Coblentz (Ph.D., Cornell, 1903) has been a physicist with the National Bureau of Standards since 1905. He is a five-time medallist: the Potts medal of the Franklin Institute; the Janssen medal of the Paris Academy of Science; the Scott medal and premium of Philadelphia; the Rumford gold medal of the American Academy of Arts and Sciences; and the Ives medal of the American Optical Society.

MODERN equipment for measuring the spectral absorptive, emissive, and reflective properties of substances consists of three parts: an assortment of powerful sources of thermal radiation; an assortment of sensitive radiometers for measuring the intensity of the total or the spectrally dispersed radiation from these sources, including equipment for magnifying the response of the radiometer and automatically recording it upon a sheet of paper; and a spectrometer, consisting of suitable lenses or concave reflecting mirrors for collimating these rays, and a prism or grating for dispersing the radiation into a spectrum.

Thus, a relatively inexperienced technician, after receiving a little instruction on how to operate this apparatus, is now able to trace upon a sheet of paper the spectral transmission curve of a substance in fewer minutes than it required hours with the spectroradiometric instruments available some four to five decades ago. The recent jump to a higher level of attainment in the detection and measurement of thermal radiant energy is so sudden and spectacular that, to the newcomer, it gives the impression that history is just beginning—that he has "discovered America."

In this connection the dependence of advancement in one field of investigation upon the progress in another branch should not be overlooked.

Thus, in 1916, at the Spring Meeting of the American Physical Society, in the discussion that followed the announcement of improvements in electronic amplifier tubes, a fellow-member fairly leaped to his feet to point out the application of the device in connection with a thermocouple for measuring the heat of the stars. But it required another quarter of a century of effort to produce the electronic amplifying and recording apparatus that has recently come into general use in connection with thermal radiation meters.

It is interesting and instructive to trace the bypaths leading from the original foundations upon which present-day spectroradiometry is based. Some of these paths are long in years. All are based upon the application of discoveries of properties of materials: chemical, electrical, optical, thermal, each one of which has an interesting history that is encyclopedic and is briefly summarized in early handbooks of physics.[1] More complete references are to be found in Poggendorff's[2] *Who's Who in Science,* which goes back to the very beginning, some 500 years B.C.

In retracing these bypaths to modern radiometry it is important to keep in mind that experimental science was then in its beginning; that these early researchers were untrained in laboratory methods as we know them today; also that, although then as now their interest was international, intercommunication was slow and mainly through publication. Hence the overlapping, repetition, and sometimes misdirection of effort in the early researches in radiant energy are understandable, especially in view of the obscurity of the subject and the lack of facilities for making the requisite apparatus.

One of these by-paths is the investigation of the electrical properties of matter and the foundation of the science of electricity, which began with the discoveries of the Italian physicists Luigi Galvani and Alessandro Volta at the end of the eighteenth century.

This was followed, in 1823, by Seebeck's[3] discovery of the production of a thermoelectric current on heating the junction of two metals; and his use of a thermopile of many junctions of bismuth and antimony for measuring temperatures, in place of a mercury thermometer, in establishing a temperature scale.

Modern methods of measuring thermal radiant energy, using a thermocouple or thermopile receiver to intercept the radiation, had their origin in Seebeck's experiments with immersion thermoelectric thermometers.

The first big advance in the investigation of the infrared transmissive properties of substances began half a century after the discovery of galvanism (in 1780) and about six years after the discovery of thermoelectricity, when Leopoldo Nobili,[4] professor of physics in the archducal museum at Florence (the old habitat of the Accadèmia del Cimento) published an illustrated description (dated

Reggio, Dec. 2, 1829) of a compensated thermopile consisting of 6 elements (couples) of bismuth and antimony, mounted in a circle, in a cylindrical wooden housing, with the 6 exposed junctions extending about 2 or 3 mm above the cement support. The junctions were soldered with tin and blackened with candle smoke.

To demonstrate cooling, he placed this arrangement under a bell jar and removed the air with a vacuum pump. We can appreciate his comments on this as a detector of radiant heat: *"Je dirai presque incroyable."*

In the second part of this same paper (dated Reggio, Apr. 24, 1830) Nobili[4] tells of providing Macedonio Melloni, professor of physics at the University of Rome,* with one of his thermopiles having 6 elements, which, however, did not seem sufficiently sensitive radiometrically. He therefore constructed a new thermopile consisting of a bundle of 16 couples mounted in a cylindrical metallic enclosure which was attached to a pedestal that could be turned in any direction. In this arrangement, which later became known as the "Melloni thermopile," the ends of the junctions were exposed to radiation, and a fixed conical reflector was used to increase the intensity of the radiation incident on the receiver.

Encouraged by the great sensitivity attained with the thermopile of 16 couples, Nobili constructed a thermopile consisting of a bundle of 40 couples, which was perfectly symmetrical in that both ends could be exposed to radiation. Two conical reflectors were provided that could be opened and closed at will, thus permitting a simultaneous comparison of the radiation of two objects mounted at opposite ends of his optical bench.

In view of the great sensitivity of these new thermoscopes Nobili[4] (*l.c.*, p. 234) announced that *"M. Melloni et moi"* were undertaking a series of researches which would be published soon.[5]

Seeing that these two researchers, at times, were separated only relatively short distances, even in those early days of inconvenient travel, one can envision the enthusiasm of Nobili and the 14 years younger Melloni in making these investigations.

In this connection it is to be noted that Nobili[6] invented also the astatic combination of magnetic needles, whereby he greatly improved the sensitivity of his galvanometer.

* According to Poggendorff,[2] Melloni was professor of physics at the University of Parma from 1824 to 1831, when, during the political revolution in Modena, Parma, and the Papal States, he escaped to Paris, where he continued his researches until 1839 when he was called to the University of Naples.

The objectionable feature of having the unexposed junctions directly back of the exposed ones (the receiver) was recognized by Nobili,[7] who already in September 1834 described a new linear thermopile, consisting of a single line of 4 "extremely small" elements of Bi-Sb, mounted sinusoidally and crosswise, the central line of receivers (details not described) slightly overlapping but not touching; total height 15 mm; covered with a movable slit, opened 0.55 mm, for studying diffraction.

But Nobili was almost eighty years ahead of his time. For suitable thermopile material all he lacked, to be modern, was pliable bismuth wire,† which became an article of commerce about 1910, and pliable antimony-tin wire, which came some years later. It is therefore understandable why Nobili's new type of linear thermopile did not come into general use until almost eighty years later.

Nobili's[5] investigations in collaboration with Melloni were published in 1831, after the latter went to Paris. His untimely death in August 1835, at the age of fifty-one, occurred at an interesting period in infrared radiometry when Melloni,[8] using a thermopile of 27 couples, of bismuth and antimony (forming a square receiving surface of 4.24 cm^2), discovered the high transparency of rock salt to infrared radiation. Thereupon he made the first prism of this material and, using a linear thermopile of 15 couples of bismuth-antimony, mounted on a crude graduated circle (but no lenses or mirrors), explored the infrared solar spectrum (*l.c.*, p. 366 and Plate 3). In another arrangement Melloni used a prism and lens of rock salt, in a stationary position, and moved the thermopile straight across the spectrum.

Nobili did not describe the difficulties in making (casting) the bars of bismuth and antimony. Melloni[8] speaks of the oxidation of liquid antimony, and the great fragility of both metals, which were in the form of bars 32 mm long, 2.5 mm broad, and 1 mm thick. The great heat capacity of the junctions retarded thermal equilibrium; this was accelerated somewhat by thermal conduction to the "cold" junctions that were directly back of the exposed junctions.

In a subsequent paper Melloni[9] gives an illustration of the assembled thermopile, galvanometer, and other accessories, which is copied in earlier handbooks of physics[1] and is familiarly known as the "Melloni apparatus."

† "In itself a remarkable achievement," as Professor E. F. Nichols expressed it, when he wrote me for the loan of a sample and inquired where such wire was obtainable.

If we are bewildered by the sudden vista of new problems presented by the recent accomplishment of atomic fission, we can envision the reactions of investigators at the end of the eighteenth century: of the British William Herschel on observing that his mercury thermometer was heated when held just outside of the visible red end of the spectrum of sunlight; of the American Benjamin Thompson, later known as Count Rumford, while engaged in boring cannon, trying to reconcile his observations of the heat developed with the then-prevailing idea that heat is a material substance; and of another American, John William Draper,[10] who unknowingly made the first observations on the radiation of what is now called a black body when he heated various metals and nonmetals in a gunbarrel and found that all became luminous at the same temperature.

The foregoing citations are only a few of the steps leading to the development of modern spectroradiometry.

SOURCES OF RADIATION

The earliest investigators had only sunlight for use as a powerful source of visible and what we now recognize as near infrared radiation.

Included in the terrestrial sources, the Leslie cube of hot water, the Bunsen burner (invented in 1835), and a blackened sheet of copper (also an incandescent spiral of platinum), heated in the flame of an alcohol lamp, were used; also the Drummond lamp (consisting of an oxy-hydrogen flame impinging upon a solid rod of chalk or zirconium oxide); and, much later, the carbon arc lamp. After 1880 the carbon filament incandescent lamp was available.

A convenient fairly reliable source of infrared radiation, in the form of the Welsbach gas mantle, composed of thorium oxide (for strength) and a small amount of cerium oxide as an emitter of light, did not come into use until 1890; and a powerful source of ultraviolet radiation, in the form of a high-pressure "hot quartz" mercury arc lamp, for laboratory use, did not become available before about 1905.

Nevertheless, it is remarkable what was accomplished in studying the optical properties of matter, using these sources of radiation. Indeed, as will be noted presently, by using different sources of luminous and nonluminous radiation Melloni found that the transmission of radiation through rock salt did not depend upon the source, indicating, sort of intuitively, a marked difference in spectral energy emission of the source; and, as noted above, he promptly assembled a combination consisting of a slit, a lens and prism of rock salt, and a thermopile which he moved linearly through the spectrum. But, because of the low sensitivity of his thermopile, he continued his investigations of the transparency of substances to infrared radiation by using the undispersed radiation of four sources, consisting of a Locatelli (wick, oil) lamp, without a glass chimney; a spiral of platinum heated to high incandescence in an alcohol flame; a thin, blackened copper plate, similarly heated to 390° C; and a Leslie cube of blackened copper heated with boiling water.

RADIOMETERS

The early measurements of radiant heat were made by noting the expansion of air in a blackened glass bulb or a pair of glass bulbs, connected with a capillary tube containing a short column of mercury, known as the Leslie (also Rumford) differential thermometer.

The first great advance in the measurement of thermal radiation came in 1829 when Leopoldo Nobili[4] constructed the first bismuth-antimony thermopile, which he combined with his improved galvanometer with astatic needles, discovered in 1825.[6]

After further improvements in the astatic galvanometer and in the thermopile, by Nobili[7] in 1834 and by Melloni[9] in 1835, this device continued, for years, to be the most serviceable radiometer for lecture room demonstrations of the properties of radiant energy, and for researches in thermal radiation.

In 1857 Svanberg[11] observed that a blackened spiral of copper wire, 0.21 mm in thickness, forming one arm of a Wheatstone bridge, when exposed to the radiation from his hand, or the walls of the room, produced a deflection of the galvanometer needle. Svanberg considered the possibility of using his galvanic differential thermometer in place of the Melloni thermopile (the name by which it was popularly known). But, like the thermopile, the area of the receiver was too large, and it was too insensitive for measuring narrow bands of spectral radiation.

In that respect the Langley[12] bolometer, developed almost half a century after the thermopile, and consisting of a thin, narrow strip of platinum, which forms one arm of a modified Wheatstone bridge, remains to this day one of the most sensitive, quick-acting radiometers ever devised for precise spectral radiometry.

Other types of thermal radiant energy meters of high sensitivity, which came into use between 1887 and 1897, were improvements, by Pringsheim[13] and

by Nichols,[14] in the Crookes[15] repulsion radiometer; also the Rubens[16] thermopile of copper-constantan (also of iron-constantan), and the Boys[17] radiomicrometer, all of which instruments were studied and intercompared by Coblentz.[18]

The high sensitivity of the present-day (thermocouple) radiometer is attained mainly by connecting it with recently developed electronic or optical devices that amplify the response of the receiver.

For the early beginnings in producing a sensitive radiometer we have to thank Leopoldo Nobili, the modest professor of physics in the archducal museum at Florence who, in 1825, while still a captain of artillery, in presenting a description of his new astatic galvanometer[6] before the academy of sciences of Modena, concluded by saying (*l.c.*, p. 125): "I do not attach great importance to this idea; but merely to indicate a means whereby 'avec le temps,' meteorology may be enriched with a new instrument." Incidentally, in the same volume in which Nobili[4] describes his new thermopile, in two papers dated a month earlier (Nov. 1, 1829), this gifted experimenter[19] gives a theoretical and experimental analysis of the electrophysiological effects upon the nerve of a frog; and he proposes to treat the two maladies paralysis and tetanus by means of electricity; the former by inducing artificial tetanus by the action of an interrupted current, and the latter by the action of a continuous current to benumb the nerve. This appears to be the forerunner of one branch of present-day (electro) physical therapy.

SPECTROMETERS

As already noted, the forerunner of spectroradiometry was Melloni's[8] systematic measurement of the diathermancy of a large number of substances to 4 sources of radiation—2 luminous and 2 nonluminous. This disclosed the great transparency of rock salt to the infrared radiation emitted by all these sources (Tr = 92%), thereby indicating the way to spectroradiometry with lenses and prisms of rock salt.

Although these early (1834) spectral radiation measurements of Melloni[8] with a lens and prism of rock salt showed the possibilities in using dispersed radiation, the low intrinsic sensitivity and the great width (2.5 mm) of the thermopile receiver militated against the use of a spectroscope. Hence, even as late as 1866 a species of filter radiometer was used to isolate wide bands of spectral radiation.

As employed by Tyndall,[20] this consisted in the measurement of spectral bands of radiation isolated by a filter; for example, a solution of iodine in carbon bisulphide, which has a wide band of high transmission extending from about 1μ to 3μ in the infrared.

Considered historically, progress in the production and measurement of spectral radiation has been by leaps and bounds. It begins with Isaac Newton's spectral dispersion of sunlight by means of a crude glass prism, way back in 1666. Then there is a leap of 134 years to the time when, on placing a sensitive mercury-in-glass thermometer in different parts of the solar spectrum, William Herschel[21] observed a rise in temperature of 2° in the violet, increasing to 7° in the red and to 9° in the infrared. By means of his concave speculum metal mirrors he showed that the infrared rays (as we now call them) can be reflected, like the visible rays; and even with these crude methods he studied the spectral transparency of numerous materials.

Groups of scientists, like a flock of sheep, are inclined to browse in the same field. Groping along blindly, they made observations several years before and after Herschel's measurements that did not agree with his as to the location of the maximum heating; some finding the maximum in the visible, others finding the maximum in the invisible, part of the spectrum. This was clarified by Seebeck,[22] who explored the solar spectrum with the blackened glass bulb of a Rumford differential air thermometer and found that the position of the maximum heating in the solar spectrum depended upon the composition of the prism (crown or flint glass), and that maximum effect is obtained when the prism is kept at minimum deviation.

In the meantime, investigators were busy with the determination of the refractive properties of liquids, glasses, quartz, etc. It was then realized that part of the disagreement in the location of the maximum heating in the solar spectrum, as observed with prisms of different kinds of glass, was because the dispersion is not uniform with wave length. Mathematical formulas were set up correlating refractive indices and wave lengths. Using sunlight reflected from a speculum metal heliostat (but no lenses) and passed through prisms of crown glass and of rock salt, Müller[23] examined the infrared solar spectrum with a thermopile, and found that it extended to 1.77μ or 4.8μ, depending upon the formula used in making the calculations. He was the first to convert his energy measurements from a prismatic to a normal spectrum (using a graphical method, later amplified by Langley); he found the maximum emission in the visible, which must be considered accidental, though true as later established by Langley's refined measurements.

Furthermore, as is now evident from the Lang-

ley bolographs, the disagreements in the measurements made by these early experimenters (some finding that the solar spectrum terminated at about 1.1μ, others observing solar radiation out to 1.7μ) occurred because of the almost complete absorption of solar radiation by atmospheric water vapor in the region of 1.1μ, 1.4μ, 1.9μ and beyond 2.6μ, depending upon the solar height (the season, and air mass traversed). This would be accentuated in the high latitudes where the observations were made.

Looking back at all this groping, and what appears (in many instances) misdirection of effort, it is to be noted that as early as 1817 Fraunhofer[24] mounted his prism on a theodolite having achromatic glass lenses, thus forming what is now called a spectrometer. Yet, as just noted, even as late as 1858 Müller[23] muddled along through the solar spectrum without using any lenses.

As already mentioned, the only powerful source of radiation then available was the sun. Presumably these early investigators realized that part of the uncertainty in their measurements of the distribution of energy in the infrared solar spectrum was the lack of achromatism of their rock salt lenses.

For a heliostat mirror these early investigators used speculum metal. Concave mirrors of speculum metal would have been too heavy, and glass mirrors backed with tinfoil-mercury amalgam would have been impracticable. After 1835, when Justus von Liebig succeeded in producing brilliant deposits of silver on the surface of glass (by heating formaldehyde with an ammoniacal solution of silver nitrate), it was possible to make mirrors with silvered surfaces. Incidentally, this was the result of Liebig's search for a substitute for the then common household mirror made of tin-mercury amalgam, the manufacture of which was poisonous to the processor.

The preparation of glass mirrors backed with a film of silver is a relatively simple process, but the deposition of a clean, tightly adhering, highly reflecting layer of silver on the surface of a plate of glass is a tedious process that did not attain high perfection until after 1880.

I have never seen an explanation of the great delay in the use of a spectrometer made of silvered concave glass or speculum metal mirrors. Presumably it was because of the great weight of the metal and the expense of making a suitable spectrometer axis; also, the expense and difficulty of producing (and conserving) silvered surfaces, which were not covered when not in use and hence deteriorated rapidly. Of course, evaporated metal surfaces were unheard-of in those days.

In 1872 Draper[25] used a Silbermann speculum metal mirror in his heliostat, and a concave glass mirror (silvered on the front surface) rigidly mounted on a table to reflect different parts of the solar spectrum upon the thermopile. This was accomplished by manipulating a series of shutters, and proved to be a poor arrangement.

A few years later a spectrometer, consisting of a prism and lenses of rock salt, was used by Jacques[26] in measuring the distribution of energy in the spectrum of various sources of radiation. He had much difficulty in maintaining good optical surfaces.

These experiences mark the early beginnings of the second epoch in the measurement of spectral radiation, when there was an increasing awareness of the necessity of making improvements in spectroradiometry.

In view of the fact that gratings were engraved on concave mirrors of speculum metal, it is understandable why similar mirrors of speculum metal were used, and why silver-on-glass mirrors came into use so slowly, especially in a smoke-laden city.

Between the years 1880 and 1886, while still at the Allegheny Observatory, Pittsburgh, Langley measured the spectral and total radiation from the moon; also the radiation from the sun and artificial sources. For this purpose he used his newly invented bolometer[12] as a radiometer in combination with silvered mirrors and spectrometers with a Roland grating;[27] also with lenses and prisms of rock salt, or of a glass that was especially diathermanous out to 2.5μ. After coming to Washington (in 1888), in addition to his duties as secretary of the Smithsonian Institution, Langley built an Astrophysical Observatory, introducing innovations in spectroradiometry that were far ahead of his time.[28]

Among other improvements these included: (1) a large Foucault siderostat mirror of glass, silvered on the front surface; (2) a large concave silver-on-glass mirror on a fixed pedestal to project and focus the solar spectrum upon the bolometer; (3) a large rock salt prism (19 cm high) with a large, flat, silvered mirror, mounted on the axis of rotation of his spectrometer (the mirror keeping the beam at minimum deviation as the solar spectrum traveled across the bolometer); and (4) a photographic registration of the galvanometer deflection resulting from the heat absorbed by the bolometer in different parts of the solar spectrum.

Since the incident solar beam is practically parallel, only one mirror is needed as used by Langley and his successors. With his rock salt prisms, figured by his friend John Brashear, Langley was

able, for the first time, to obtain a fine resolution of the D lines in the solar spectrum.

The first use of mirrors instead of lenses in a laboratory spectrometer was made in 1883 by Pringsheim,[29] who combined two fixed silvered mirrors of plano-concave glass with a Rutherford grating mounted on a spectrometer axis which was rotated to focus the spectrum on a Crookes radiometer.[13] With this apparatus he explored the infrared solar spectrum to 1.5μ. Although continued cloudy weather prevented him from making an extensive series of measurements, enough was accomplished to point the way to building mirror spectrometers, by skilled technicians, for general laboratory use.

Thus, soon after 1890 the second epoch of infrared spectroradiometry began. It marks the first real beginning of a quantitative investigation of the spectral infrared absorptive, emissive, reflective, and refractive properties of materials, which has continued for over half a century. That the discovery of the spectral absorptive properties of materials would have a practical application could not then be foreseen. Or, if foreseen, this method of spectrochemical analysis would have been too costly. Furthermore, the real need did not arise until three or four decades later.

About 1940 we entered the third epoch of spectroradiometry. The practical methods of application of the information on instruments and methods of spectroradiometry and on infrared absorption spectra of substances, accumulated during all the intervening years, have developed by leaps and bounds in recent years, and have attained a high state of perfection in a remarkably short time. The regrettable thing is that this sudden advance could not come in peaceful surroundings, and be used wholly for peaceful purposes.

REFERENCES

1. JAMIN, M. *Cours de Physique*. (4th ed., M. BOUTY.) Paris: 1891; WÜLLNER, L. C. *Handbuch Experimental. Physik*. (5th ed.) Leipzig: 1895, **2**, 192; PRESTON, T. *Theory of Heat*. London: 1894; KAYSER, H. *Handbuch der Spectroscopie*. Leipzig: 1900, **1**; RUBENS, H. *Rapports, Congrès Internat. de Physique*. Paris: 1900, **2**, 141.
2. POGGENDORFF, J. C. *Biographisch—Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften*. Leipzig: 1863, **1, 2**; 1898, **3**.
3. SEEBECK, T. J. *Pogg. Ann. der Phys.* 1823, **6**, 1; *Denkschr. Berlin Akad. Wiss.* 1822–23.
4. NOBILI, L. Description of a *Thermo-Multiplicateur* or Electric Thermoscope. Bibliothèque Universelle. *Sci. & Arts*, Geneve, 1830, **44**, 225.
5. NOBILI, L., and MELLONI, M. Recherches sur Plusiers Phénomènes calorifiques Entreprises au Moyen du Thermomultiplicateur. *Ann. Chim. et Phys.*, 1831, **48**, (2), 187.
6. NOBILI, L. Description of a New Galvanometer with Two Needles. Bibliothèque Universelle. *Sci. & Arts*, Geneve, 1825, **29**, 119.
7. NOBILI, L. Description of Two New Thermoelectric Piles and Their Use in Calorific Researches. Bibliothèque Universelle. *Sci. & Arts*, Geneve, 1834, **57**, 1.
8. MELLONI, M. Memoir on the Transparency of Different Solids and Liquids to Infrared Radiation. *Ann. Chim. et Phys.*, 1853, **53**, (2), 5; 1834, **55**, 337.
9. MELLONI, M. *Pogg. Ann. der Phys.*, 1835, **35**, 112.
10. DRAPER, J. W. On the Production of Light by Heat. *Phil. Mag.* 1847, **30**, (3), 345.
11. SVANBERG, A. F. Galvanic Differential Thermometer, *Konigl. Svensk. Vetensk. Akad. Handl.*, 1849, 109; *Pogg. Ann. der Phys.*, 1851, **84**, 411.
12. LANGLEY, S. P. Actinic Balance. *Amer. J. Arts & Sci.*, 1881, **21**, (3), 187.
13. PRINGSHEIM, E. Crookes Radiometer. *Wied. Ann. der Phys.*, 1883, **18**, 1.
14. NICHOLS, E. F. New Type Crookes Radiometer. *Wied. Ann. der Phys.*, 1897, **60**, 401.
15. CROOKES, W. Repulsion Radiometer. *Proc. Roy. Soc. London*, 1874, **22**, 32.
16. RUBENS, H. Thermopiles. *Z. für Instrumentenkunde*, 1898, **6**, 65.
17. BOYS, V. Radiomicrometer. *Proc. Roy. Soc. London*, 1887, **42**, 189.
18. COBLENTZ, W. W. Instruments and Methods of Radiometry. *Bull. Bur. Stand.*, 1908, **4**, 391; 1912, **9**, 7; 1914, **11**, 131; 1918, **14**, 507; 1921, **17**, 187.
19. NOBILI, L. Experimental and Theoretical Analysis of the Electrophysiological Effects. . . . Bibliothèque Universelle. *Sci. & Arts*, Geneve, 1830, **44**, 48, 165.
20. TYNDALL, J. On Calorescence. *Philos. Trans. Roy. Soc. London*, 1866, **156**, 1; *Contributions to Molecular Physics in the Domain of Radiant Heat*. London: Longmans, Green, 1872.
21. HERSCHEL, F. W. Experiments on the Refrangibility of the Invisible Rays of the Sun. *Philos. Trans. Roy. Soc. London*, 1800, **2**, 255, 284, 293; **3**, 437.
22. SEEBECK, T. J. Ueber die ungleiche Erregung der Wärme im prismatischen Sonnenbilde. *Abhandl. der Berliner Akad. d. Wiss.*, 1818–19, 305; 1822–23.
23. MÜLLER, J. Wellenlänge und Brechungsexponent der äussersten dunklen Wärmestrahlen des Sonnenspectrums. *Pogg. Ann. der Phys.*, 1858, **105**, (4), 337, 543.
24. FRAUNHOFER, J. Determination of Refractive Indices of Different Glasses. *Gilbert's Ann. der Phys.*, 1817, **56**, 264.
25. DRAPER, J. W. On the Distribution of Heat in the Spectrum. *Amer. J. Sci.*, 1872, **4**, (3), 161; *Philos. Mag.*, 1872, **44**, 104.
26. JACQUES, W. W. Distribution of Heat in the Spectrum of Various Sources of Radiation. *Proc. Amer. Acad. Arts & Sci.*, 1879, **14**, 142.
27. LANGLEY, S. P. The Experimental Determination of Wave Lengths in the Invisible Prismatic Spectrum. *Memoir, Nat. Acad. Sci.*, 1883, **2**, 149.
28. LANGLEY, S. P. *Ann. Astrophys. Obs.* Washington: 1902, **1**.
29. PRINGSHEIM, E. Infrared Solar Spectrum. *Wied. Ann. der Phys.*, 1883, **18**, (3), 32.

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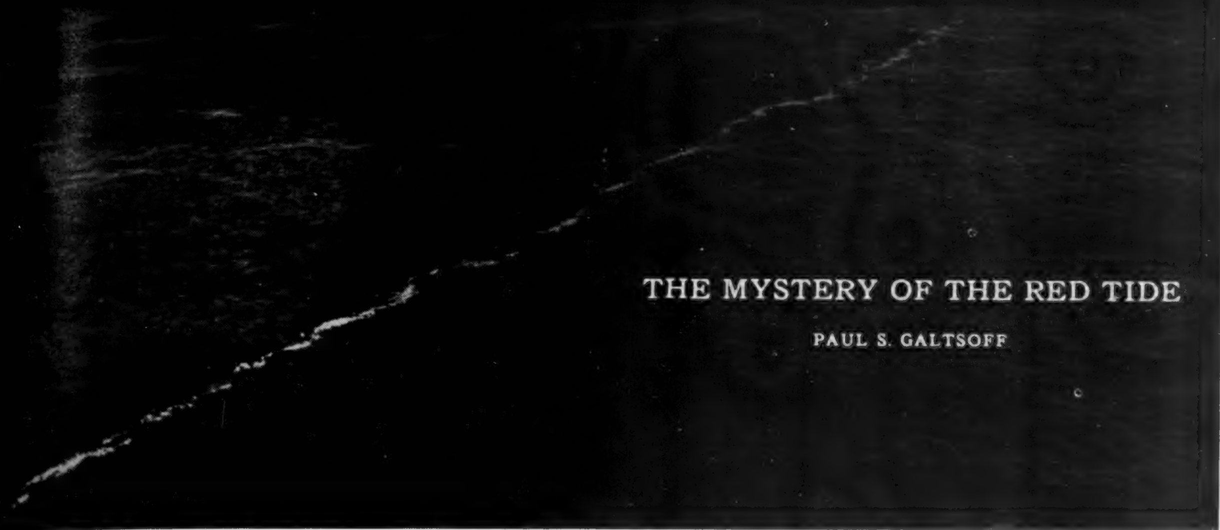

THE MYSTERY OF THE RED TIDE

PAUL S. GALTSOFF

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THE blue color of the open spaces of the ocean, like the color of the clear sky, is a physical phenomenon caused by the scattering of light against particles smaller than the shortest wave length of the visible spectrum. This explanation, consistent with present scientific evidence, implies that light may be scattered either by the molecules of water itself or by highly dispersed minute particles suspended in it. The pure and highly transparent water of the open sea contains no special coloring substance that could give it its remarkable hue.

As one approaches the shore the change in color and in the transparency of water becomes noticeable. Dark-blue shades give place to greenish or yellowish coloration and the water becomes more turbid. The presence of a water-soluble "yellow substance" derived from numerous small algae (phytoplankton), which thrive in coastal areas, causes the change in color, and pigment-bearing microorganisms, which abound in plankton, greatly increase its turbidity. The muddy appearance of large stretches of the sea near the mouths of big rivers is due to mineral particles brought in with land drainage. Water containing a large amount of suspended material, which changes its normal blue color, is called "discolored."

Pronounced discoloration of water caused by rapid multiplication or by swarming of microscopic organisms is often referred to as "blooming." Although it may frequently be observed in freshwater ponds and in the reservoirs supplying our cities with drinking water, blooming rarely attracts the attention of the layman. It does, however, provoke his indignation, and bitter complaints against the "oily" flavor of drinking water (caused by the presence of diatoms, flagellates, and green and blue-green algae), or against the unpleasant taste resulting from chemical treatments used to destroy these undesirable although harmless organisms. The incidents of the blooming of the sea are more rare. They usually excite public curiosity and concern, especially when the water along the beaches or in the vicinity of coastal towns becomes red and the discoloration is accompanied by mortality of fish, shellfish, birds, and other denizens of the sea.

The exact conditions that provoke blooming are not known. Under normal situations there exists in the sea a balance between the populations of different species of animals and plants comprising its plankton. The equilibrium is rather unstable, for the ratio between the various organisms is not fixed. It changes almost daily and may easily be upset by a rapid growth of a population of a single species, almost to the exclusion of all others. If the rapidly multiplying organisms contain green, red, or other pigment, the color of the water changes accordingly.

Above: Line of dead fish, photographed from the air at about 500 feet altitude, marks the path of the red tide off the Florida coast. (*St. Petersburg Evening Independent* staff photo.) *Left:* Dead fish floating in water, as seen from low-flying plane. (*St. Petersburg Times* photo.)

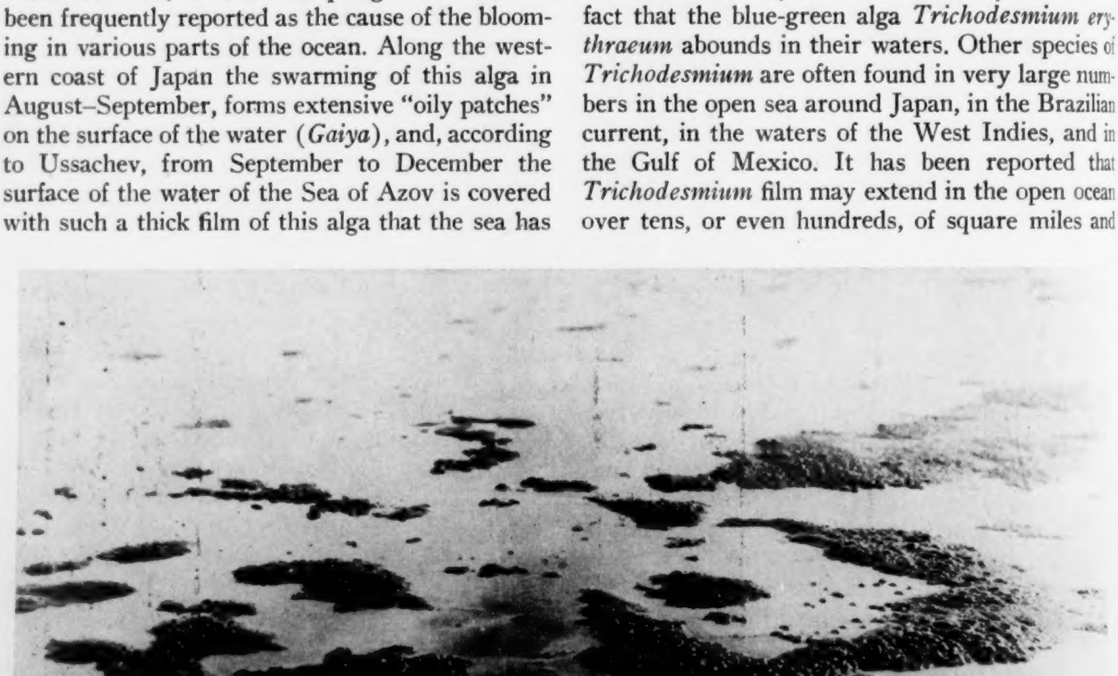
* Article published by permission of the Director, U. S. Fish and Wildlife Service.

An excellent illustration of blooming is the repeated rapid propagation of a diatom, *Aulacodiscus kittoni*, along the shores of Copalis Beach, in the state of Washington. Outbursts of rapid reproduction of this diatom may occur several times during one year. They can be easily observed from the shore. Persons who have studied the phenomenon report that under certain combinations of meteorological conditions the growth of the diatom is so rapid that within a few hours brown patches of an almost pure culture of it appear on the surface of the sea. They are carried by the waves and deposited on the beach in layers several inches thick. Blooming of *Aulacodiscus* usually lasts for only a few days and has no harmful effect on fish or shellfish. After it has ceased, normal equilibrium between the various species of plankton is re-established until the next outburst, which apparently occurs after light rain. The exact conditions that favor rapid growth of the *Aulacodiscus* population are, however, not known. It is interesting to note that in this case the blooming is restricted to a rather narrow surf zone several hundred feet wide, and does not extend farther offshore.

Rhizosolenia, a common pelagic diatom, has been frequently reported as the cause of the blooming in various parts of the ocean. Along the western coast of Japan the swarming of this alga in August–September, forms extensive "oily patches" on the surface of the water (*Gaiya*), and, according to Ussachev, from September to December the surface of the water of the Sea of Azov is covered with such a thick film of this alga that the sea has "an appearance of a placid swamp with typical odor and dark brown color."

Swarming of a colonial flagellate of the genus *Phaeocystis* is often responsible for brownish discoloration of water in moderate latitudes. Several species of this slime-covered alga, whose colonies are visible to the naked eye, have different temperature tolerances. The range for *Phaeocystis poucheti* is between 1.0° and 11.6° C, whereas *Ph. globosa* prefers warmer water—between 6.3° and 16.7° C. Swarming of *Phaeocystis* is of great practical significance to fishermen in the North Sea, for the alga is extremely offensive to herring and they are never found in the areas where *Phaeocystis* is abundant (Savage).

Dark- and bluish-green water is usually caused by the blue-green algae (Myxophyceae). The cells of these plants contain two pigments: the blue-green phycocyanin and the red phycoerythrin. The proportions of the two pigments vary in different species, some of them having only one pigment. The preponderance in plankton of the species rich in phycoerythrin may cause red discoloration. Thus the names of the Red Sea and of the Vermillion Sea (Gulf of California) reflect the fact that the blue-green alga *Trichodesmium erythraeum* abounds in their waters. Other species of *Trichodesmium* are often found in very large numbers in the open sea around Japan, in the Brazilian current, in the waters of the West Indies, and in the Gulf of Mexico. It has been reported that *Trichodesmium* film may extend in the open ocean over tens, or even hundreds, of square miles and



Masses of a diatom, *Aulacodiscus kittoni*, deposited by surf on tidal flats of Copalis Beach, Washington.

that its presence, which can be detected from the deck of a ship by a noticeable odor of chlorine, calms down the waves (Delsman). The growth of blue-green algae in the brackish waters of the Azov and Baltic Seas is so prolific during the summer that the surface of the sea "has an appearance of a green meadow" (Knipowitch).

Red discoloration of sea water may be caused by a great variety of organisms. Swamps along the seashores of Denmark are often full of red water because of the prolific growth of purple sulphur bacteria (*Chromatium, Thiopedia,* and *Thiocystis*), and the red water of the Sicilian "Lake of Blood" is caused by the luxurious growth of *Thiopolycoccus ruber, Thiopedia rosea,* and other species (Forti). *Rhodothece pendens, Rhabdomonas rosea,* and other sulphur bacteria color the sea near Helgoland "as though with rose-red milk of sulphur" (Ellis). Several species of sulphur bacteria are commonly found in the lagoons along the northeastern coast of the Black Sea and in marine bottoms off the northern coast of European Russia (Issatchenko).

Red color caused by sulphur bacteria is apparently harmless and has no effect on marine populations. A serious situation results, however, from the outburst of propagation of various species of dinoflagellates, which frequently cause red or brownish discoloration of large areas of the sea. In many instances the red color of the water caused by these microorganisms occurs simultaneously with mortality of fishes and oysters.

One of the earliest published records of red water, or red tide, is that of Charles Darwin, who, during the voyage of H. M. S. *Beagle* in 1832 off the coast of Chile, took a sample of water which had a "pale reddish tint" and, examined under a microscope, "was seen to swarm with minute animalculae darting about and often exploding."

Along the coast of Peru the frequent occurrences of red water are invariably accompanied by extensive fish mortality and liberation of H_2S. Locally, the phenomenon is known as *aquaji*. Gunther, who studied the plankton and the discoloration of water in this area, states that the outburst of red water is provoked by a sudden rise in temperature and convergence of the abnormal countercurrent known as El Niño. The exact cause of the Peruvian red water has not yet been ascertained, but it is probably due to dinoflagellates, for Gunther maintains that the orange-colored water contained large numbers of flagellates with red pigment.

Similar occurrences of red water, frequently associated with fish mortality, have been reported from the Malabar Coast of India, from Australia, various places in Japan, South Africa, the west coast of the United States, British Columbia, the Gulf of Mexico (west coast of Florida) and Narragansett Bay. Unfortunately, nowhere has systematic study of the red-water phenomenon ever been made; the observers usually record the time and location, occasionally give temperature and salinity data, and sometimes state the identity of the microorganisms responsible for discoloration.

Japan has paid more attention to the study of the red tide (*akashiwo* in Japanese) than any other country, probably because of the extensive damage caused by red water to valuable pearl-oyster grounds. According to Aikawa, between 1899 and 1934 there were twenty-four recorded occurrences of discoloration of water in Japan, sixteen of which were accompanied by mortalities of fish and shellfish. Of these instances, three were caused by *Noctiluca,* nine by other dinoflagellates, five by diatoms, and one by blue-green algae. The cause of the others was not determined. Ago Bay (34°17′ N. and 136°47′ E.) was the place of the most frequent outbursts of red tide, which appeared there seven times, causing some damage to pearl oysters and inflicting particularly severe losses in the winter of 1933–34. Next in frequency was Tokyo Bay, where discoloration was recorded three times. The records summarized by Aikawa show that in Japan red tide may take place at any season of the year.*

Along the coast of California, near La Jolla, red discoloration of water is of common occurrence. It was reported for the years 1906, 1907, 1917, 1924, 1933, and 1935. The zone of discoloration sometimes covers a large area extending for about three miles along the coast. Red water is often observed along the seacoast of the state of Washington and of British Columbia. Undoubtedly there are many other instances of red tide that remain unrecorded.

Red discoloration is also caused by swarms of *Noctiluca,* a naked dinoflagellate widely distributed in the Atlantic and Pacific Oceans; by dense populations of a marine ciliate, *Mesodinium rubrum*; by local concentrations of medusae (*Aurelia* and various siphonophores); by copepods (*Calanus finmarchicus*); and various species of crustacea of the group of Euphausiidae, which constitute the principal food of whales. There is no evidence that any of these forms are responsible for mass mortality of fish.

Several species of armored and unarmored dino-

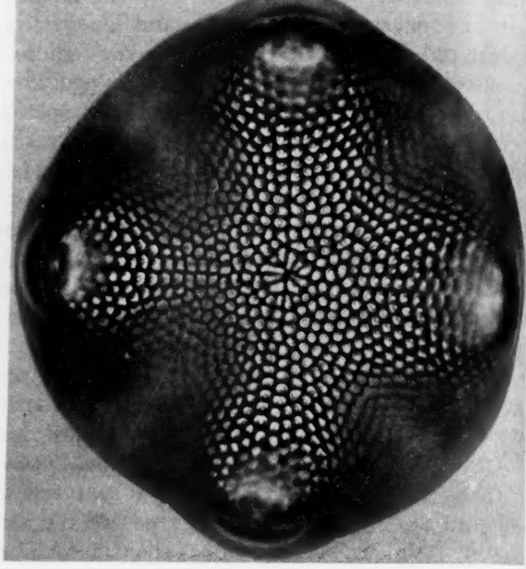
* I gratefully acknowledge the courtesy of Dr. Charles J. Fish, who made available to me abstracts of Japanese publications translated in his office.

flagellates listed as the cause of red water require particular attention. *Gymnodinium splendens, G. sanguineum, G. mikimoti, Cochlodinium catenatum, Polykrikos schwartzii, Pouchetia rosea, Gonyaulax polygramma,* and *G. catenella* have been frequently observed in Japan; *Peridinium triquetra* in S. Africa; *Prorocentrum micans* in California; *Gymnodinium splendens* and *Gonyaulax alaskensis* in Puget Sound; *Glenodinium rubrum* in Australia; *Amphidinium fusiforme* in Delaware Bay; and *Gymnodinium brevis* along the west coast of Florida. In the absence of a comprehensive study, one may consider that the list is not complete and that some of the identifications made in the field are only tentative. Some of the above-listed dinoflagellates are apparently nontoxic. Thus, there is no evidence on which to suspect *Amphidinium fusiforme, Glenodinium rubrum, Polykrikos,* and *Pouchetia.* The evidence against the others is mostly circumstantial, the accusations being based on the fact that the species was predominant in the red water at the time of mass mortality of fish or shellfish. The proof that several species of *Gymnodinium* and *Gonyaulax* are toxic is provided, however, by both field observations and laboratory experiments.

The rate of propagation of any unicellular organism that multiplies by division is very rapid. The division of each mother cell gives rise to two daughter cells, which in turn repeat the process at regular intervals. Since there is no death of the parent cells, the unicellular organism reproducing in this manner may be considered potentially immortal. The growth of the population of such a form increases with geometrical progression and may be expressed in a general formula, $2^{tp} = N_t$, where t is the number of days since the beginning of propagation, p is equal to the number of generations per day, and N_t is the total number of individuals comprising a population on a given day after the beginning of propagation. The formula is employed by Wernadsky to express the universal property of living matter, namely, its ability for unlimited expansion. The formula does not take into consideration the mortality caused by accidental death or predation by other organisms. Under actual conditions in the sea, these two factors are of great importance, for they provide the mechanism that maintains an equilibrium between the various members of the planktonic community.

With favorable physical conditions and abundant supply of nutrient material, the growth of a population of marine protozoa may be extremely rapid, approaching its theoretically possible rate. The space available for the expansion of a growing population in the sea is almost unlimited, and the reserve of nutrient material may last for several weeks, or even months. Eventually, changes in the temperature and in other factors of the environment, and the exhaustion of the food supply, slow down the process and the blooming gradually ceases.

An increase in the total number of autotrophic organisms in the sea water cannot occur without



Aulacodiscus kittoni.

an adequate supply of nutrients. Plankton samples collected during the outbreak of red water off the Florida coast showed that red discoloration was invariably accompanied by the predominance in plankton of *Gymnodinium brevis* (new species, described by Davis). Several species of diatoms, copepods, marine Cladocera (Evadne), and lamellibranch larvae were also present, but in the patches of red or brownish water the *Gymnodinium* was far more abundant than all the rest of the planktonic organisms put together. Woodcock estimated during one of the field trips that samples of deep-red water contained as many as 56 million *Gymnodinium* per liter. The water was slimy with mucus, apparently derived from broken *Gymnodinium* bodies.

Since *Gymnodinium* is autotrophic, it would be logical to conclude that an additional quantity of nutrient salts, and particularly of phosphates, became available to sustain its rapidly growing population. Partial confirmation of such an assumption is found in the chemical analyses of samples of red water made by Ketchum, of the Woods Hole

Oceanographic Institution. He found that surface samples collected in a red-water area contained from five to ten times as much total phosphorus as had ever been encountered in the oceanic waters uncontaminated by land drainage or domestic pollution.

Two explanations suggest themselves. High content of total P may be due to the swarming of *Gymnodinium* in the upper stratum, whereas the propagation of this organism may have taken place in the entire column of water. In this case the phosphorus extracted from the deeper layers was simply transported to the upper level. On the other hand, if the propagation of the *Gymnodinium* was restricted to the surface layer, it means that there was an additional supply of phosphates in the sea. Present observations do not answer the question but point to the necessity of a comprehensive study of vertical and horizontal distribution of total phosphorus and other nutrients in the area affected by red water.

The possible source of the additional supply of phosphorus is of great interest. The southern half of Florida abounds in phosphate rocks that are extensively mined. Large quantities of phosphate, used as fertilizer, are loaded from a platform on one of the islands near the Fort Myers area and shipped by water. Whether there exists a way by which the phosphate deposits are washed away or in other manner reach and fertilize coastal waters can be determined only by further studies. At present, further speculation along these lines does not seem to be justifiable.

The occurrence of discolored water along the west coast of Florida in 1946–47 can serve as an illustration of a typical outbreak of poisonous red water. Unfortunately, at the first signs of mortality, in the latter part of November 1946, no competent biologist was engaged in any field studies in this section of the coast, and the beginning and spread of mortality can be reconstructed only from oral or written reports of fishermen and owners of shore properties. About November 20, mackerel fishermen noticed dead or dying fishes in the patches of red or brownish water fourteen miles offshore from Naples. The "Red Tide," as it was immediately called, spread northward, reaching in December and January the inshore waters near Sanibel and Captiva Islands. Fish continued to die, and millions of dead carcasses were floating in the water or were cast ashore. All the beaches in the Fort Myers area became littered with dead bodies of fish, which accumulated at the rate of more than 100 pounds per linear foot of shore line. In February 1947, carcasses of fish were washed ashore on Englewood Beach, marking the northernmost extent of mortality; Cape Romano appeared to be the southernmost limit. The strip of discolored water was confined to the coastal zone, extending from five to eight miles offshore. Beyond this area the sea water was clear and of normal dark-blue hue.

According to the observation conducted by the Marine Laboratory of the University of Miami, all kinds of animals perished in the red water, including a small number of turtles and porpoises (Gunter, Williams, Davis, and Smith). A great many common commercial and noncommercial fishes were recognized in windrows piled on beaches or among the carcasses floating in the water. Likewise, the pelagic and bottom invertebrates succumbed to the unknown poison. Large numbers of dead shrimp were noticed, as well as common blue crabs, fiddler and mud crabs, barnacles, oysters, and coquinas. Observations made by me in March 1947, around the Fort Myers area, disclosed that about 80 percent of the edible oyster *Ostrea virginica*, grown on piles, were dead. Clean inner surfaces of their shells, free from any fouling organisms, indicated that death had occurred only recently. No mortality was observed among the hard-shell clams, *Venus mercenaria*, and no reports were received of the destruction of ducks, gulls, and other birds inhabiting the inshore waters, although later on some of the residents of Largo, Florida, told me of thousands of sea gulls and pelicans "which died from eating the fish poisoned by the red tide." Unfortunately, there was no opportunity to verify these statements.

Red water gradually disappeared, and normal conditions were restored in March 1947, but a few scattered outbreaks of red discoloration and fish mortality were reported in April from Key West, Marathon, and Cape Sable.

Early in July 1947, the red tide reappeared in the same place. On or about July 6 the streaks of red water, three to six feet wide and about one hundred feet long, were observed along the beaches near Venice, Florida, and in areas to the south. Within a week the area of discolored water extended from the tide line to approximately fifteen miles offshore. With the surface current it slowly spread northward and by July 30 reached Anna Maria Key, South of Tampa Bay. By August 5 the red danger extended about fifteen miles north of Clearwater. Fish killed during the second outbreak probably exceeded the number destroyed during the first one. An accurate estimate is impossible, but considering the numbers washed up on beaches and the carcasses carried out by the

Mass of dead fish deposited at edge of water at high tide. (Photo by John Evans, *St. Petersburg Times.*)

tide, it is highly probable that from one to two hundred million pounds of fish were destroyed. Tightly packed masses of dead fish in bands one hundred to two hundred feet wide and extending over a great distance along the coast were observed several miles offshore. W. Anderson, of the U. S. Fish and Wildlife Service, who made field observations at the time of summer mortality, states that when flying or sailing over the affected areas "one was seldom out of sight of dead fish."

Spectacular and disastrous as it was, the outbreak of the red tide in Florida was not without precedent. Since 1844, the year of the earliest record, the discoloration of sea water, accompanied by an extensive mortality of fish along the east coast of the Gulf of Mexico, has been reported several times. One of the more recent outbreaks, in 1916, in the Fort Myers area, fully described by Taylor, will be discussed later.

In the mind of the layman, mass mortality of fish appears to be convincing evidence of the poisoning of water by some unknown and powerful chemical dumped into the sea, or by release of poisonous gases from the bottom as a result of an earthquake or volcanic disturbance. The fact that red water contains myriads of living and rapidly multiplying microorganisms contradicts this popular belief. It is reasonable to expect that water containing strong chemicals deliberately dumped into it, or poisoned by volcanic gases, would be much more toxic to delicate unicellular organisms than to fish, shellfish, and crustacea, which in general have greater resistance. Deprived of plankton the sea water would appear crystal clear and highly transparent. We know, however, that red water is very turbid and overloaded with plankton.

One might suspect that death of fish or shellfish in red water is due to suffocation, caused by the occlusion of their gills. This explanation, often advanced, is not confirmed by my observations during the recent outbreak of red tide in Florida. The gills of dead mullets and thread herrings collected in red water near Fort Myers, Florida, were not covered by any foreign material, and the internal organs appeared to be normal.

Frequently, references are made to the odor of H_2S emanating from red water. These statements lead to the belief that the death of fish may be due to the lack of oxygen. It is true that decomposition of great masses of fish and plankton may exhaust the supply of oxygen—but field observation shows that this takes place only in limited areas, close to the shore. No deficiency in oxygen content of water sufficient to be detrimental to fish was found in the affected area in Florida.

Judging by the description given by fishermen and residents along the sea beaches, death of fish entering red water comes rather suddenly. As the fish (mullet, for example) enters the patch of red water it begins to act rather strangely, coming to

the surface, whirling around, then turning on its side or floating stomach up, and finally sinking to the bottom. This suggests that some unknown substance in red water specifically affects the nervous centers controlling equilibrium and respiration.

There is convincing evidence that this substance is derived from plankton. Experiments performed by the Miami Laboratory showed that sheepshead minnows (*Cyprinodon variegatus*) died in 22–46 hours after being placed in a tank filled with water containing live *Gymnodinium brevis*, whereas there was no mortality among the fishes placed in a similar tank filled with water from Biscayne Bay, in which *Gymnodinium* was absent.

A series of experiments performed by me at the Woods Hole Laboratory proved that a toxic substance can be extracted from red plankton by acidified alcohol. The residue, redissolved in sea water, was lethal to killifish (*Fundulus heteroclytus*) in a concentration of 1 : 250, in which the fish died within 1 hour 25 minutes. *Fundulus* placed in this solution soon loses equilibrium; it turns on one side and unsuccessfully attempts to right its body. Irregularity of respiratory movements and floating with belly up appear after a more prolonged exposure and are followed by death if the fish is not removed from the poisonous solution.

Less pronounced effects, shown only in a loss of equilibrium, are apparent even in the dilution 1 : 1,000. Greater dilutions (1 : 2,000) of red extract and extracts prepared for comparison from plankton consisting mostly of diatoms collected in Woods Hole Harbor were harmless.

The effect of the red-water extract was also tried on small toadfish (*Opsanus tau*) and on young horseshoe crabs (*Limulus polyphemus*). In all these cases the test animals perished in a short time, the time of their survival depending on the concentrations used.

Red-water extract was found also to have a pronounced effect on the development of sea urchin (*Arbacia*) eggs. By using a portion of the same sample of red water that I used to prepare the extract, Corman has demonstrated that in 1 : 5 dilution of red water the rate of cleavage of sea urchin eggs was retarded by 27 percent and that the eggs were cytolized in 1 : 2 dilution. Comparing his results with my data, Corman thinks that there may be some parallel between the toxicity of red-water extract to *Fundulus* and the retardation of cleavage of *Arbacia* eggs by the untreated sample.

Visitors to the coast of California are familiar with the signs prominently displayed on rocks and along the beaches warning them against the consumption of mussels from the first of May to the end of November. The reason is that during this period the mussels may be poisonous. The symptoms of mussel poisoning in man or animal may appear immediately after the consumption of toxic shellfish. They are entirely of nervous origin and begin with a prickly feeling, followed by numbness in the lips, tongue, and fingertips. The progress of the intoxication is accompanied by muscular incoordination with ascending paralysis. In severe cases death from respiratory failure may occur 2–12 hours after the consumption of the fish. Gastrointestinal disturbances are rare, although vomiting may develop in severe cases.

During the periods of low toxicity of mussels small amounts of poison are frequently ingested with the shellfish meat without any ill effect on humans. The mussel poison is, however, the strongest one known to science and one for which no antidote has yet been discovered. The substance is stable in acid or neutral solution, withstands heat, but can be gradually destroyed by boiling in alkaline solution. The exact chemical nature of mussel poison has not been determined, but it is known that the toxic principle belongs to the class of alkaloids such as strychnine, muscarine, and aconitine. Since the poison has no odor or flavor, ordinary inspection will not distinguish poisonous shellfish from normal ones.

The presence of poison is usually established in the laboratory by making tests on white mice or kittens. Prevention seems to be, therefore, the only safe method of protection. This consideration led the Health Departments of the Pacific Coast states to establish a quarantine for mussels in summer months and at such other times as laboratory tests show them to be dangerous for human consumption. Research conducted with the support of the William Hooper Foundation of the University of California provides evidence that the original source of mussel poisoning is a plankton containing large numbers of *Gonyaulax catenella*, a dinoflagellate that thrives during the summer months and frequently is the cause of the red water along the coast of California.

Like other lamellibranchs, mussels feed by filtering large quantities of water. They ingest the *Gonyaulax* and store its poisonous substance in the digestive gland without suffering any ill effect themselves. The *Gonyaulax* theory is based primarily on the correlation between the rise and fall of mussel toxicity and the increase and decrease in the abundance of *G. catenella* in coastal water. The toxicity of mussels completely disappears shortly after the disappearance of this microorganism.

Direct evidence of the toxicity of another species

Bulldozers were used to bury dead fish littering Florida beaches. (*St. Petersburg Independent* staff photo.)

of dinoflagellates was demonstrated in 1938–39 by H. J. Koch, who showed that mussel poisoning in the marine canal between Bruges and Zeebrugge, Belgium, was caused by the ingestion by these mollusks of a microorganism described by Woloszynska and Conrad as *Pyrodinium phoneus,* n. sp. Taking advantage of a strong positive phototropism of this dinoflagellate, Koch collected large quantities of *Pyrodinium* and extracted from them a highly toxic substance which killed white mice in 90 seconds. Furthermore, normal mussels placed for 18 hours in water to which Koch added known numbers of *Pyrodinium* acquired strong toxicity, directly proportional to the concentration of the dinoflagellate.

The question of whether the toxic principle of red water, so destructive to fish, is identical with the paralytic shellfish poison, or whether we are dealing with two different chemicals, remains unanswered. Samples of Florida red water which yielded extracts toxic to fish were sent by me to the Food and Drug Administration of the Department of Agriculture for bioassy. The results were negative, however. Likewise, no shellfish poison was detected by the Fisheries Technological Laboratory in College Park in clams grown near Fort Myers in the area affected by red water. Present observations are not yet sufficient to arrive at a definite conclusion regarding this question.

The outbreak of the red tide in Florida in 1946–47 was accompanied by a strange phenomenon which greatly added to the discomfort and distress of the residents of the beaches and islands of the coastal area. With the onshore wind and breaking of the surf an odorless but highly irritating "gas" emanated from the water. It caused spasmic coughing, a burning sensation in the throat and nostrils, and irritation of the eyes. Local residents, experimenting with samples dipped from the sea, observed that strong coughing was produced by inhaling vapors emanating from the water heated over the kitchen range. For several days, when onshore winds persisted, life on Captiva and other islands of the affected area was very uncomfortable. Virtually the entire population was sneezing and coughing and suffering from other symptoms resembling those of a heavy cold or hay fever. The disagreeable experience strengthened the general belief that poisonous gases, dumped into the sea, were coming from the water. The "gas" did not, however, affect persons living a few hundred feet from the beach.

The occurrence of an irritating substance in dis-

colored sea water was not entirely new. It was reported from the same area by Taylor, who investigated the mortality of fishes on the west coast of Florida in October–November 1916. Taylor writes that "the gas was very violent and many people telephoned for medical assistance for 'cold in the head,' 'sore throats,' 'cold in the chest,' etc. I myself, have suffered quite acutely for the past five days, but the worst of the gas seems to be going now." Another occurrence of the "poisonous gas" coincident with a mortality of fishes was recorded in Texas by Lund in 1934. Woodcock, who studied the transfer of water droplets over the air in the red-water area in Florida, suggested that the irritating substance may be projected into the air when small bubbles burst on the surface of breaking waves. Indirect evidence in support of this view was found in the following tests. Spraying into the nose of a small amount of sea water containing 15–56 million *Gymnodinium brevis* per liter caused coughing and sneezing and produced a burning sensation in the nose and throat. A similar reaction was produced by inhaling samples of red water heated to about 80° C or slightly higher. At this temperature, clouds of fine bubbles rise to the surface and burst. The irritating effect ceases as soon as heating and bursting stop. Tiny water droplets arising from heated red water were caught by Woodcock on the surface of a microscope slide covered with hydrophobic silicon film. Examining the droplets under the microscope, in a special chamber which prevents evaporation, I found that they contained yellowish granules, in shape and color similar to the granules found in the cells of *Gymnodinium*. It is tempting to infer that these droplets contained the irritating substance, but more detailed study is needed before this question can be settled.

Is it possible to control the outbreak of red tide and prevent the mass destruction of fish and other marine forms? The question may be answered with certain reservations. Although control of any condition in the sea presents great difficulties, it may be not entirely impossible. Our ability to understand the cause of a phenomenon and to foresee its occurrence is the first step toward solving the problem of its control. Several attempts to stop the red tide, or at least to mitigate its effect, were made by Japanese scientists. Taking advantage of the known fact that dinoflagellates are very delicate creatures and can be killed by low concentrations of poisons, they applied weak solutions of copper sulphate, calcium hypochlorite, and free chlorine to destroy them in harbors and bays. To protect the valuable pearl oysters from contact with the poisonous plankton, they used simple mechanical methods and covered the oyster beds with matting. In the absence of detailed reports, it is difficult to appraise the practical results of these efforts. It is claimed by the Japanese, however, that they were successful.

From a purely theoretical point of view, chemical control of the red tide appears to be feasible. Red patches of water containing rapidly growing populations of dinoflagellates can be easily spotted from the air. By spraying or dusting them from a specially equipped airplane, growth of the dinoflagellates probably may be stopped, or at least localized. It appears doubtful, however, that any control would be possible after the red water has spread over many hundred square miles of the open ocean.

The red tide may be considered as a special case of the blooming of the sea. The study of this phenomenon leads into interesting and but little explored fields of science. It may reveal intricate and very complex conditions which determine the numerical relationship between the different marine populations and therefore greatly increase our knowledge of marine ecology. The human health aspects of the phenomenon are fascinating because we are dealing with such a powerful poison. Studies of its origin, chemical composition, and mode of action may be the objects of fruitful scientific investigation and lead to important discoveries. Let us hope that explorations along these lines, which can be undertaken only by a large research organization, will no longer be neglected.

MEDICAL RESEARCH: OPERATION HUMANITY

ANDREW C. IVY

Vice president, professor of physiology, and head of the Department of Clinical Science at the University of Illinois (Chicago), Dr. Ivy (Ph.D., Chicago, 1918) has taught at Loyola, Chicago, and Northwestern Universities. His article is based on an address delivered during the Centennial Celebration of the AAAS, September 13–17, 1948.

MEDICAL research is a clear and simple example of "One World of Science." The knowledge derived from such research is directly applied in the practice of medicine, a field of human endeavor unified throughout the world. Medicine is the only world-wide profession that pursues the same humanitarian aims and uses the same methods everywhere. It possesses such catholicity of method, and there is such a universal demand for its services, that the physician can render his service in the same environment in every country in the world.

This universality is natural because, first, disease is a curse on all living things. It attacks all persons, irrespective of their position in society. When the methods of prevention and cure of a disease are truly known, they are universally applicable for the eradication or amelioration of the disease. Second, next to the conquest of hunger, the conquest of disease has been of greatest concern to all peoples. The benefits derived from the conquest of power and space, and the production of more comfortable living conditions, are empty luxuries without health to enjoy them. Third, sympathy for the sick, suffering, and dying is a widely felt sentiment, and it is everywhere recognized that the physician has reverence for the life of the individual and, through the individual, for the life of the community and of all humankind. The moral obligation of the medical and allied professions to provide assistance to the suffering has been respected by every civilized group except when insane fanaticism has suppressed all moral restraints. In warfare, the doctors, nurses, medical corpsmen, and Red Cross workers of belligerent nations are classified as noncombatants. It is understood that the participation of medical personnel in hostilities is for the purpose of salvaging, not destroying, life. Amidst havoc and conflict, medicine is now the only scientific agency that knows no distinction between friend and foe when disease and suffering are at hand. It is clearly recognized throughout the world that medicine and medical research constitute an operation for humanity, and for humanity alone.

THE UNITY OF MEDICINE AND SCIENCE

But medicine and the other branches of science are inseparable and have been so since the beginning. Simultaneously, the mysterious nature of disease and of the heavens stimulated the mind of primitive man, giving rise to medicine and astronomy, both closely associated with his religion. Medicine, like astronomy, was the mother of many of the natural sciences. And it was a physician who first introduced and adopted the scientific process, or method, called in medical research the Hippocratic method.

The progeny of medicine and astronomy—chemistry, physics, and mathematics—have made tremendous contributions to medical research and the cure and prevention of disease, and there can be no doubt that progress in medical science is intimately related to progress in all the natural sciences. In fact, all branches of learning and creative effort move forward together. Moreover, it cannot be repeated too frequently that social and governmental attitudes may deter or facilitate progress. Governments and governmental agencies throughout the world should keep in mind that the doctrines of totalitarianism suppressed the free functioning of the human mind and entombed the gifts of Hippocrates, Aristotle, and Galen for a thousand years during the Dark Ages. This dogmatic authority had to be broken before the natural sciences and the humanities could again go forward together.

Thus, the factors involved in human progress are seen to be so interdependent that every group that has fostered creative and cooperative effort has contributed to the advancement made during the past hundred years.

A CENTURY OF PROGRESS IN MEDICAL RESEARCH

The tremendous reduction of human suffering and in the loss of human lives due to disease

since 1848, is great cause for rejoicing, but the results of the application of science to the prevention and control of suffering have become so commonplace that many of us have lost our appreciation of them. Since the founding of the AAAS, vaccination, anesthesia, aseptic surgery, investigation of the causes and the methods of control of infections and parasitic diseases, radium, the X-ray, the electrocardiograph, sulfa drugs, aviation medicine, antibiotics, insect control, and the sciences of sanitation and nutrition have been introduced. In addition, phenomenal results have attended experimental studies that have been conducted in every civilized region of the world. Researches in the physiological, biochemical, pathological, bacteriological, and histological fields have rendered it possible to make diagnoses of disease and to apply surgical and medical treatments undreamed-of even as short a time as fifty years ago. These, the greatest humanitarian victories of science, have been won by the zealous efforts of countless investigators in many nations.

These achievements are dramatically demonstrated by the following facts: Life expectancy in 1850 was approximately forty years; in 1900, it was forty-nine years; and in 1945, sixty-six years. Thirty percent of persons who have now attained the age of sixty-five owe their survival to advances made in medicine since they were born. The advances made since 1900 have added five years to the life expectancy of those now forty years of age. Thus, not only have millions of lives in the United States and other countries been saved through reducing the number of deaths in childhood, but years of life have been added to those who pass the age of forty.

Mortality statistics of the United States Army from both world wars illustrate the advances of medical research during the past thirty years. The death rate from disease declined from 14.1 per 1,000 in World War I to 0.6 per thousand in World War II. The percentage of deaths among the wounded decreased from 10 to 3 percent.

There are numerous medical problems yet to be solved, such as mental disease, alcoholism, and the degenerative diseases. But those who recall how yellow fever, smallpox, cholera, plague, diptheria, and other contagious diseases once killed hundreds of thousands of people, those who work with and for the sick and suffering, those who measure progress by the greatest happiness for the greatest number will agree, I believe, that the prolongation of life and the alleviation of suffering have been the greatest gifts of science to man during the past century.

SOME OLD AND NEW PROBLEMS

These achievements in the field of medical science have magnified some old and created some new problems:

I. The Problem of Degenerative Disease and Old Age.

II. The Problem of Population and Hunger.

III. The Problem of Biological Warfare.

It is at once obvious that these problems have important medical, social, political, economic, and moral aspects. And, on the basis of the history of the success of the natural sciences and the relative failure of the social sciences, or the failure to develop a science of humankind, the medical aspect of these problems appears to be the least difficult.

I. *The Problem of Degenerative Disease and Old Age.* We have reached a turning point in the history of medical research. The emphasis in the past, and properly so, was on the control of acute diseases, which, except for the virus diseases, have now largely been more or less checked. In the future the emphasis must be on the alleviation of chronic degenerative diseases.

The importance of this problem may be illustrated by the following figures. Almost 29 percent of the people in the United States are over forty-five years of age; by 1960, 33 percent will be over forty-five years of age. Almost 11,000,000 persons are now over sixty-five years of age (7.7 percent); by 1960, 9 percent of the population will be over sixty-five years of age. The shift of our population from youth into old age will probably occur more rapidly than is now predictable, because the mortality from pneumonia, tuberculosis, and other infectious diseases is rapidly decreasing. This shift in age will obviously increase the number of people with degenerative diseases and chronic illnesses. Even now degenerative diseases, such as chronic heart disease, hardening of the arteries, chronic nephritis, and cancer, account for about 6 percent of the deaths in the United States. Thirty percent of the patients admitted to mental hospitals are sixty years of age or more, and most of these patients have senile psychosis because of cerebral arteriosclerosis. One and a half million people have the degenerative form of arthritis. Seventy percent of persons now disabled from all causes are forty-five years of age or more.

Unless, through medical research, we discover a way to decrease the disabilities incident to chronic illness and old age, our economy and our social organization as we know them today will have to undergo many changes. One fourth of the population will be working to take care of the other

three fourths. This emphasizes the importance of planning productive work for the aged. When a person is forced to retire because he cannot keep up with the production line, he should not be placed on the unemployable list. Work he can do should be found for him. Observation of the aged indicates that sudden inactivity speeds aging and the time at which custodial care is required.

Thus, medical research has been creating an obligation for future generations to care for proportionately many more old and disabled persons than are being cared for at present. The only way this obligation can be lightened by medical research is to give more attention to the cause and prevention of degenerative disease.

The next important contribution to be made by medical research is "to prolong useful life." Most informed medical scientists feel certain that this phase of the medical problem can be solved reasonably well given sufficient time and financial aid. Medical science, in which I include psychology, can give sound advice to the social sciences regarding the solution of the economic problem of old age by measuring aptitudes and abilities for employment.

II. *The Problem of Population and Hunger.* Today approximately 175,000 infants are born daily. The number has been increasing during the past hundred years, and the percentage of survivals has been steadily increasing as a result of the application of the findings of medical research. In 1830, the population of the world was 800 million; in 1900, 1.6 billion; and in 1939, 2.2 billion. At the present rate of increase, and with the spread of medical science, the population of the world will be 4 billion by the year 2000.

Is this a pleasant prospect? Not when we remember that a surplus of food has never existed in the world, that to provide everyone with a diet of 2,600 calories a day during the next twenty-five years the world's food production will have to be doubled. This does not seem possible when there are only 4 billion acres of land that can be used for food production by present methods, and when 2.5 acres of farm land are required to produce an adequate diet for one person. This serious situation is made worse when we consider that practically nothing is being done in the present food-producing areas of the world to prevent soil erosion and depletion. Soil erosion and depletion caused the transformation of garden spots into deserts in Greece, Syria, Northern Italy, Africa, Mesopotamia, and the uplands of China; today we hear of dust storms in the Volga Valley, in South Africa, Australia, Argentina, and the United States, the "breadbaskets" of the world; and dust storms mean soil erosion and soil depletion. The following question has recently been asked by C. L. Walker (*Harper's Magazine,* Feb., 1948): Shall man's food and population problem continue to be resolved by famine, disease, malnutrition, and wars caused by peoples in need of living room?

This population problem, obviously, has been and will be further magnified by the application of the results of medical science. It must be solved by the social sciences and the humanities. Medical science can provide information regarding the nutritional requirements, and perhaps science can find some nonagricultural way of producing food, but it would seem urgent *now* to check soil erosion and depletion in the still-existing agricultural areas of the world and to discourage population increases. It seems certain that one way to decrease the rate of population growth is to increase living standards; in the past an increase in living standards has been associated with a decline in birth rate, and the same will probably hold true in the future.

III. *The Problem of Biological Warfare.* Knowledge of the infectious diseases that has made it possible to save lives and to protect the production of food may soon be used to destroy human, animal, and vegetable life. Nowadays we read or hear almost daily that biological knowledge—like chemical and physical knowledge—threatens civilization. I have not seen or heard of anyone who seriously doubts it, and it would seem obvious that knowledge may be used for good or for evil purposes. The possibility of the destruction of man by his own inventions was known to the classical Greeks. In the field of medicine, Hippocrates saw the problem and provided a solution. He clearly realized that the scientific and technical philosophy of medicine represented by him and his school could not survive without a sound moral philosophy. The moral code he formulated has been a controlling influence in determining the policies of the medical profession and the conduct of the physician for twenty-two centuries.

SCIENCE AND TECHNOLOGY ARE NOT ENOUGH

We have reached the point in the development of science and civilization where it is clear that they cannot survive without a sound moral philosophy. Science and technology are not enough. In themselves they have no survival value unless used as tools for the attainment of some great

altruistic goal for the good of all mankind. There is no lack of a goal toward which the energies of all humanity may be religiously directed. The conquest of hunger and disease has not yet been accomplished. Pestilence and famine still exist in many parts of the world, and famine promises to increase in the future unless a world-wide effort is begun now to prevent it.

Hunger and disease are common enemies of all mankind. It is high time that the people of the world join in an effort to improve the food-production potentialities of the world and to eradicate pestilence. Men must think *first* of their common enemies in nature and unite to bring these enemies under control. The natural enemies of man will win the conflict unless men cease wasting their energies and intelligence fighting among themselves.

Medicine and medical research are moving in the right direction through the World Medical Association and the World Health Organization. It is the duty of these organizations to eradicate pestilence. All the natural sciences must rally behind the Food and Agriculture Organization, whose duty it is to bring hunger everywhere under control.

Finally, I want to take this opportunity to direct an appeal to the political and military leaders of the nations of the world.

The results of medical research and the efforts of medicine have directly and indirectly saved the lives and increased the happiness of millions of people. People everywhere know that for centuries medicine has worked to promote human welfare. This moral obligation of medicine and its allied professions has been universally recognized. People everywhere have faith in the fact that their physicians, even under conditions of warfare, are sworn to conserve life, and that the function of medical research is to prevent and cure, and not to cause, disease.

Until recently there never has been any question of directing medical research and knowledge toward any objective other than the improvement and preservation of human life. But it has lately been proposed that bacterial warfare might be used *aggressively*. In fact, I have seen Nazi documents in which a committee reported that America could be conquered only by afflicting the people, the food, animals, and plants with disease. It is very important to increase our knowledge regarding methods of preventing all contagious diseases. It is important for all nations to energetically promote research toward the provision of a complete defense against bacterial warfare. But I plead with those political and military leaders who would use bacterial warfare as an aggressive weapon to ponder the ultimate effect on themselves, and on their own people, combatants and noncombatants alike, of crushing the faith that all people have in the humanitarian spirit of medicine. Woe unto the society that would crush and not cherish and cultivate this spirit among its people, medical scientists, and physicians.

Those who have little faith in the effectiveness of such a plea may gain consolation from the fact that the development of a strong defense against bacterial warfare will render its use unlikely. The best way that medical research can make certain that its knowledge will not be used by predatory men against the welfare of man is to prepare a complete defense. Although this may not be possible in chemical and atomic warfare, it is entirely feasible in the case of biological warfare.

Past and present religious leaders, educators, and scientists have made pleas for unity, peace, and concord. Thus Pasteur in 1888 called for an end of war and destruction and urged adherence to "the law of peace, work and health whose only aim is to deliver man from the calamities which beset him." Recently Albert Einstein has stated that "World government can come through agreement or through the force of persuasion alone, hence at low cost. . . . It will not be enough to appeal to reason. . . . Unless the cause of peace based on law gathers behind it the force and zeal of a religion, it hardly can hope to succeed."

In these two statements we have the unifying principle that will cultivate the growth of the peace-producing forces and encourage understanding all over the world. The calamities of disease and hunger stir the emotions of all people. The energies and emotions that governments marshal to win a war against a human enemy should be as readily marshaled against hunger and disease. The creation of harmony between man and his enemies in nature—disease and hunger—will go far toward creating harmony between man and man. The efforts of medical research, the natural sciences, the social sciences, and the humanities—of all who are interested in the spiritual and material welfare of man—will be required for the success of this Operation for Humanity.

AN IMMIGRANT CONQUERS A CONTINENT: THE STORY OF THE WILD GARLIC

HUGO ILTIS

Dr. Iltis (Ph.D., Prague, 1905) has been director of the Gregor Mendel Museum at Mary Washington College of the University of Virginia, Fredericksburg, for the past ten years. In May 1943 THE SCIENTIFIC MONTHLY *published his article on "Gregor Mendel and his Work," which has since been widely reprinted.*

ONE of the first botanical observations I made after emigrating from my native Czechoslovakia to Virginia was the common occurrence, even prevalence, of European weeds along roads and near human habitations. Whereas in Europe only a few American weeds have become full-fledged naturalized citizens—such as *Erigeron canadense* in waste places, *Erechtites hieracifolia* in clearings, the South American *Galinsoga parviflora* in gardens, *Elodea canadensis* in pools and rivers, etc.—in America scores of different species of European roadside flowers and field weeds almost displace the native ones in some regions. The explanation seems to be simple enough: the European settlers brought to America not only the seeds of most cereals, but also those of different weeds; whereas only a few cultivated plants were taken from America to Europe, and the seeds of those few (corn, tobacco, tomatoes, etc.) were not too likely to be contaminated with many weed seeds.

The strangest encounter, however, was my meeting with one of the most troublesome American weeds, *Allium vineale* L., the wild garlic, or the wild onion, as it is called in other localities. I remembered immediately that years ago I had had to walk several miles from my native Brünn just to get a few specimens of this plant for our herbarium. How had the plant which was of scattered or rare occurrence in Europe become a common weed in America? I first looked in several European floras to confirm my memory. Hegi, in his *Flora of Central Europe,* writes about the distribution of *A. vineale* L.: "Here and there in vineyards, on sandy fields, sunny hills, on ridges, lawns, roadsides. . . ." Potonie's *Flora of North and Central Germany,* 1910: "Scattered, mostly on sandy hills and fields, rare." Wildt's *Botanical Excursions Book for the Surroundings of Brünn* mentions just one single locality where *A. vineale* L. is to be found.

It is, however, somewhat different at the circumference of its native area. In some of the older English floras, *A. vineale* L. is not considered to be a weed. J. D. Hooker, in his *Student Flora of the British Islands,* 1884, for instance, writes: "*A. vineale* L.—Crow Garlic—pastures and waste, dry places, from the Clyde and Aberdeen south, not frequent. . . ." But it became a troublesome weed in England and in southern Scandinavia in modern times, as is shown by recent publications of R. H. Scott (Life History of the Wild Onion and its Bearing on Control. *J. Min. Agric.,* London, 1944, **51**) and R. H. Scott-Richens (1947). The little map in the second paper shows an area in south-central England where *A. vineale* L. became a serious weed. The earliest record Mrs. Richens could find was in Trow's *Flora of Cardiff,* where it was stated to be serious in the vale of Glamagan, South Wales, at the end of the eighteenth century. It is of little importance in that locality now. In regard to its occurrence as a weed on the European continent, Mrs. Richens writes in a letter to the author: "Sweden is the only country I know of where it is taken seriously. I rather think that a maritime habitat is its natural one and that it has spread from there, becoming a serious weed whenever conditions were favorable." For a taxonomic group "center of the area" refers—after Cain—to the center of its origin, that is, to the territory where the dispersal and the immigration began. Its determination for *A. vineale* would be difficult at the present state of our knowledge.

There are only small, restricted areas in its European home country where *A. vineale* L. is a troublesome weed, but it has become one of the most obnoxious weeds ever introduced into the middle Atlantic and south-central parts of the United States. From Europe it was brought to the United States in the seventeenth or the early part of the eighteenth century. The "*Allium arvense odore gravi, capitulis bulbosis rubentibus,*" as it is referred to in Gronovius' *Flora Virginica* (1739, p. 32), is very probably identical with our species. We know from the investigations of F. J. Pipal that by the middle of the eighteenth century it was in full possession of

many farms in eastern Pennsylvania. In the first volume of the *Transactions* of the American Philosophical Society, Henry Hollingsworth, of Philadelpia, writes: "In April, 1754, I perceived the wheat much colored with garlic, and at harvest found in many places parts of the field almost every tenth head was garlic, which rendered the wheat unfit for use. . . ." It is evident from his introductory remarks that his experience with wild garlic was not uncommon. Dr. Barton, in his *Compendium Florae Philadelphicae,* published in 1818, calls wild garlic "a common and pestiferous weed which has obtained such footing in pastures that it is impossible to extirpate it." Nuttall, in *The Genera of North American Plants* (Philadelphia, 1818), writes (p. 34): "*A. vineale* L.—naturalized, now a prevalent and injurious weed." In Gray's *Manual of Botany* (1848, p. 493) *A. vineale* L. is said to be naturalized and troublesome near the coast. C. C. Deam, in his *Flora of Indiana* (p. 309), writes: "My specimens are all from southern In-

Above, left: ***forma typicum*** Beck and ***forma capsuliferum*** Koch; ***right:*** ***forma crinitum*** Jacob showing mature plant as found in mid-June, aerial stalk carrying awned bulbils and central soft bulb surrounded by several hard-shelled bulbs.

diana where it is one of the most pernicious of all weeds. A pioneer who lived in Point Township, Posey County, told me that when he was a boy (about 1860) both the garlic and wild onion were common in the woodland."

Historical research has brought to life some especially interesting features. It has shown that wild garlic, where it had gained a foothold, was as great a problem in the United States more than a century ago as it is today. But there is also no doubt that wild garlic is slowly taking possession of a constantly increasing area in the middle United States. The present distribution of wild garlic —its "artificial area" (Cain)—ranges from Massachusetts south to northern Georgia, west to Missouri and Mississippi. It is most abundant in the belt of territory extending from New Jersey through Delaware, the Virginias, North Carolina, Kentucky, Tennessee, and along the southern portions of Pennsylvania, Ohio, Indiana, and Illinois.

Wild garlic seems to have little or no preference as to the character of site or soil. It is most widely distributed in the river valleys and creek bottoms, owing to the fact that the bulblets are carried and scattered by the water. It is also frequently found on higher, rolling land. It thrives in clayey as well as in alluvial and loamy soils. It is not characteristic of any particular kind of crop, but may be found in any of them, being most troublesome in meadows and pastures and in the wheat crop.

What are the characteristics that make the wild onion such an efficient combatant in the struggle for life? A description of the plant and its means of reproduction will supply an answer. The mature plant has a 30–70-cm high flower or aerial stalk growing out of a bulb, which is surrounded by a withered leaf, forming a thin, torn membrane. The bulb contains at maturity one large major offset, called the soft-shelled bulb. It is developed near the center and covered by a thin, slightly buff-colored, glossy membrane. It is ovate in its longitudinal section and convex on its outside, with the concave inside clasping the base of the scape. There are in addition several (1–6), mostly small, sharp-edged minor offsets with sharp terminal points enclosed by hard, straw-colored, shell-like coverings; these are called the hard-shelled bulbs. The aerial stalk coming out from the center of the bulb is in its lower part still partly covered by the dry remains of the 5–6 foliage leaves and carries the head, consisting either of flowers, of bulbils, or of both. The head shows at its bottom the remains of the 2 bracts that enclosed it before maturity. From the composition of the head taxonomists have distinguished several varieties of forms (see Britton and Brown, Fernald, etc.). There is, first, *A. vineale* L. *forma typicum* Beck, with a more or less loose umbel consisting of both bulbils and flowers; second, *A. v. forma compactum* (Thuill.), with a compact head consisting only of bulbils of a whitish or greenish color; third, *A. v. forma fuscescens* Ascherson and Graebner, with a head consisting of bulbils of a reddish color; fourth, *A. v. forma crinitum* Jacob, bulbils forming the head tipped with long, green, capillary appendages; and, finally, *A. v. forma capsuliferum* Koch, with an umbel consisting of flowers only. The flowers are pedicellate, pale rose or violet, the perianth segments tinged with pink, or sometimes with green, along the keel, this being most marked in those of the outer whorl. The capsule is elongate and trilocular, the seeds black, triqetrous, two in each loculus, but rarely all developing. *A. vineale* has (after Darlington and Janaki) 32 chromosomes. Since 16 is the chromosome number of the majority of the *Allium* species, it is apparently tetraploid. Apogamy is common in *A. vineale* (after Hunger and other authors).

The species *A. vineale* would be, after the definition given by Camp and Gilly, a parageneon, containing aberrant but interfertile genotypes, or formae. The different formae are possibly the result of mutations. The term " forma" is applied since it should be restricted to those variant individuals that occur intermixed with those of the species.

The five varieties or, better, forms described above are not dependent upon the locality or the environment but often occur together. They all occur in Virginia, the *formae fuscescens* and *crinitum* being most common, the *formae compactum* and *typicum* coming next, and the *forma capsuliferum* being the least frequent. There are, however, often transitions between these forms. To all appearances these forms seem to be dependent upon Mendelian allelic genes, and their occurrence at the same places to be like a segregation within a hybrid generation.

The wild onion has, therefore, as means of reproduction, first, the major offsets, or soft-shelled bulbs; second, the minor offsets, or hard-shelled bulbs, both underground; third, the red aerial bulbils with long green appendages; fourth, the red bulbils without appendages; and fifth, the whitish or greenish bulbils without appendages. Finally, there are the seeds resulting from sexual reproduction. Each of these six ways of reproduction is very effective. Together they give to the wild garlic all the advantages necessary to survive in the struggle for life and they make it a trouble

Varieties of the wild garlic (*Allium vineale* L.) : *Left to right: A. v. forma crinitum* Jacob, not ripe; *A. v. forma crinitum*, ripe (dry) ; *A. v. forma compactum* Thuill.; *A. v. forma typicum* Beck (two forms) ; and *A. v. forma capsuliferum* Koch.

some weed, one extremely difficult to eradicate.

Propagation of wild garlic by seeds takes place mostly near the southern limits of its range. There are no seeds formed on wild garlic in England (Scott) or in Indiana (Pipal). In Tennessee (Watts) and in Virginia, seeds are abundantly produced. Nearly 100 percent of the seeds are viable. From about a hundred seeds I planted at the end of August, the great majority (95) had soon formed a grayish-green terete leaf surrounded at its base by a yellowish or purple sheath. It was growing out from a small bulblet the size of a large wheat grain, which was fixed to the ground by 3–5 slender roots, each 1–3 inches long within two weeks. The sheath at the ground proved to be purple if the seeds had been taken from a plant with purple bulbils (*forma fuscescens*), but yellowish if taken from a plant with whitish bulbils (*forma compactum*). Of course the growth of the young plants was stopped during the winter and was resumed the next spring. In the summer following this spring (June), the bulblets had reached the size of a large pea or a small hazelnut. But only one bulblet had developed from the plants grown from seeds at the end of the first year.

The central, or soft-shelled, bulb starts its growth early in the fall and often continues to grow during the fall and the early winter months. From the soft bulb usually originates the strongest plant, which may again produce an aerial stalk. The terete gray-green leaves produced by that bulb are 8–10 inches long before the frost begins. Growth is then stopped and is resumed in the spring when the formation of new adventive underground bulbs begins. These bulbs, the new soft bulb and several hard-shelled bulbs, originate within the axils of the fleshy, scalelike leaves of the maternal soft bulb. During April and May the coverings of these new bulbs grow tough, and they become separated from the parent bulb. Each bulb develops an increasing number of roots arranged in a semicircle around the attachment scar.

The sharp-edged, hard-shelled, or lateral, bulbs do not start to germinate the same season as do the soft-shelled bulbs, the aerial bulbils, and the seeds. They sprout the next summer at the earliest. Most of them, however, exhibit a marked dormancy and do not germinate for at least two years. Tinney deduces from his experience that the maximum period of dormancy of the hard-shelled bulbs is about six years. Thus, these hard-shelled bulbs become the means by which the wild garlic may survive through unfavorable seasons, through drought and frost, and through fungus infection.

The aerial bulbils are the most effective means of reproduction of the wild garlic. Almost all mature wild garlic plants, with the exception of the rarer *forma capsuliferum*, which has umbels consisting of flowers only, produce heads or clusters of aerial bulbils. Each plant normally produces a single head, but it is not uncommon to find stalks with 2, 3, or even 4 heads. The aerial stalk bearing the heads starts to elongate in early April and emerges in May from the sheath of the innermost foliage leaf as a green, terete scape. Its tip carries the head, which is first small and completely enclosed within a membranous spathe. The spathe bursts in June, exposing one or more heads consisting of bulbils or of bulbils and flowers or of flowers only. The bulbils need about four to six weeks to ripen.

There are three well-distinguished kinds of bulbils, each kind arranged separately in different heads. The first form consists of whitish or yellowish bulbils with pointed greenish or purple tips, which are slightly bent to one side. These whitish heads do not at all—or very seldom—show the long green appendages but often contain in addition to the bulbils a small or large number of flowers. The

number of bulbils in these, as in the other forms, varies a great deal. There can be as few as 20 and as many as 150. If there are few, they are usually large (5–8 mm long); if there are many, they are small (3–4 mm). The bulbils look very much like grains of wheat or rye. Each bulbil consists of one cone-shaped, fleshy scale, which contains a growing point and which may include near its base just above the scar the first young root. Each bulbil is surrounded by a kind of coat consisting of two or three cell layers. The color of the bulbil depends on the color of the second cell layer under the epidermis of the bulbil itself. Its vacuoles are colored by anthocyanin in the purple bulbils; the vacuoles are colorless in the whitish ones. The second, more common, form consists of dark-purple bulbils similar in shape to the whitish bulbils, also without awns. The difference between these two forms is clear-cut, and there are generally no transitions present. The two characters purple and white seem to behave like a pair of Mendelian characters. The white color may depend upon a recessive gene inhibiting the formation of anthocyanin. The heads consisting of purple bulbils may also contain some flowers, although the formation of flowers occurs more commonly among the whitish bulbils.

The third kind of heads is characterized by long green appendages, sometimes 2–3 inches long. The bulbils carrying these appendages are almost always red or purple in color because of the anthocyanin in the cell layer beneath the epidermis. The appendages, which are already fully developed within the spathe before its bursting, look very much like the young leaves of the wild garlic. Some authors mistook the appendages for the result of germination of the bulbils within the head (Hegi, *Flora von Mittel Europa*, **II**, 217, *"Brutzwiebeln . . . wachsen gelegentlich bereits am Stengel aus"*). Such a head of bulbils, each one ending in a long, green, terete leaf, really resembles an inflorescence of *Poa vivipara* or of *Juncus bufonius* with their viviparous bulblets— ". . . the outer leaf shows on its tip a distinct blade thus imitating a bulbil already germinating" (Kirchner-Loew-Schroeter). But a more exact investigation instantly shows the difference. The appendage is nothing but the continuation of the coat, or cover, of the bulbil. The coat of the bulbil is purple, thin, and more or less transparent, however, and the appendages are green parenchyma rich with chlorophyll. The epidermis of these appendages is characterized by the large number of stomata. The epidermis of the green aerial stalk carrying the head of bulbils showed an average of about 10 stomata in the field of view of my microscope, whereas the epidermis of the appendages showed an average of 14–15 stomata.

This fact may help us to understand the function of these appendages. It may be similar to the function of the awn of the grasses (Zoebel, 1893) or of the elongated style of the fruits of *Geum* (Iltis, 1913). It is an organ of transpiration helping to accelerate the metabolism and the ripening of the bulbils. A preliminary experiment seems to support my hypothesis. On a waste field with several hundreds of mature specimens of the wild onion, the awns of 50 heads were cut off with scissors and 50 similarly awned heads in the neighborhood were left to ripen undisturbed. The experiment was started on June 1, and the heads were harvested three weeks later. There was no difference in the shape or size of the bulbils visible whether the awns were cut or not. There was, however, some difference in regard to the tempo of ripening. Those heads with the awn still present seemed to have

Section through aerial bulbil with green appendage (awn). The bulbil is enclosed by a coat, the continuation of which forms the awn. The bulbil is formed by a fleshy scale, which encloses the growing point. A young root is formed near the scar.

ripened more quickly. The awn became dry and brown. The drying started at the tip of the awn and progressed down to the bulbil. Finally, the bulbils dropped off, some of them singly, some in clusters. The clusters were kept together by threadlike, wrinkled, dry awns. The aerial bulbils, whether single or in whole clusters, could be carried by the wind for a short distance. But they were spread especially by rain and floods and probably by manure, since a percentage of the bulbils went undigested through the digestive tracts of birds and mammals.

The main function of both the awn and the coloring by anthocyanin seems to be to accelerate the metabolism and thus to quicken ripening. This turns out to be an advantage to the wild garlic where it is a weed in wheat fields. The bulbils ripen at the same time as the wheat, and, since they look very much like the wheat grain, they are harvested with the wheat and may be sown together. It is, however, a disadvantage to the farmer since the wheat by the admixture of garlic bulbils becomes "garlicky," decreases in value, and may even become unfit for consumption.

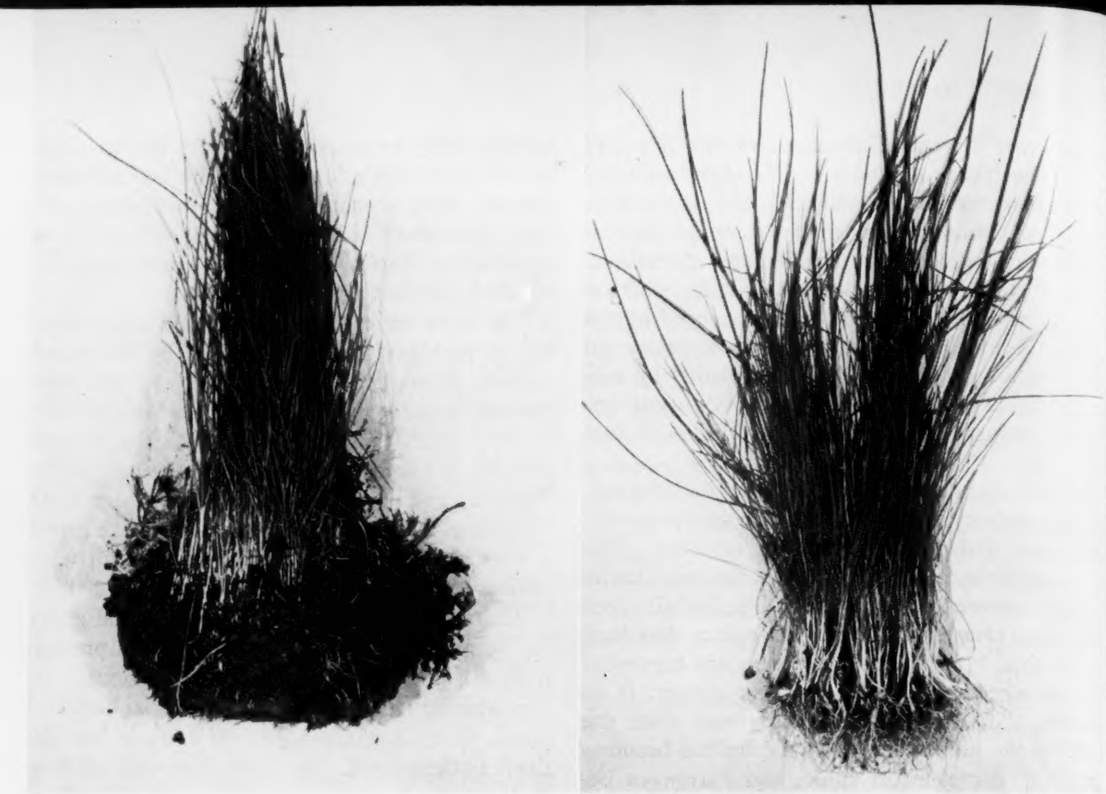
Almost all aerial bulbils are viable. Of a hundred bulbils planted on September 24, the great majority (about 90) started to germinate around October 1, forming in about a week the first gray-green, terete leaf coming out from a purple or whitish sheath. Two weeks later the second leaf was formed. At that time the bulbils themselves had increased in size and become about as large as pea seeds. Some weeks later, around the middle of November, secondary bulbils started to form at the base of the primary ones. The secondary bulbils developed between, and at the base of, the scaly layers of the primary one and grew during the winter (December) into separate plants. When the young plants were taken from the ground the following June, 2-6 bulblets had been produced by each aerial bulbil. Each one had grown into a separate independent small plant with its own root system. Each of these secondary bulblets may itself form new bulblets next year. Thus the number of young plants may be very much increased.

This method of reproduction by formation of an increasing number of secondary and tertiary bulblets from one primary aerial bulbil is especially characteristic for the very troublesome lawn variety of the wild garlic. Everyone in Virginia, and probably in all southeastern states north of Georgia, knows the brushlike bunches of green garlic leaves, 5-15 inches long, which appear everywhere on the lawns in early spring before the grass starts its growth. They disappear during the hot summer months, only to reappear again in the late fall. If we pull out such a bunch of garlic leaves from the ground after a rain we will be surprised at the large number of bulbils at its base. There are very often more than a hundred: in some instances I counted almost two hundred.

The lawn variety and the similar variety appearing in pastures are characterized by two distinct periods of growth, one in the spring and one in the fall. At some places, especially where the winter is very hard, there are also two distinct resting periods, one during the hot summer months (July, August) and one during the cold winter months (January, February). At other places, where the winter is milder, the leaves stay green all winter, although their growth may become retarded.

The wild garlic in pastures, where the grass is cut by grazing instead of by mowing, is prevented from forming aerial stalks and, therefore, from reproducing either by seeds or by aerial bulbils. Instead, the reproduction goes on scarcely less effectively underground. The rapid reproduction of wild garlic in the lawn is a mere nuisance impairing its smooth appearance. The garlic does real damage in pastures, however, since the milk from cows grazing there acquires a garlicky flavor. The cutting of

a, young plant grown from aerial bulbil, two weeks after planting (November); *b*, plant grown from aerial bulbil, six months later (June), secondary bulbils developed; *c*, small part of a bunch of young plants of the lawn variety taken from the ground in November.



The lawn variety of the wild garlic. Bunch of plants, with soil, taken in November and, *right*, the same bunch cleaned by running water, showing bulblets and roots.

the grass at regular intervals—and similarly the regular grazing of the pastures—keeps the wild garlic from reaching the mature stage, but, on the other hand, stimulates the formation of secondary and tertiary bulblets.

"There are few weeds, if any, which cause greater loss to the farmer within the region where it thrives than wild garlic," writes Pipal in his excellent investigation (Wild Garlic and its Eradication. *Agric. Exper. Sta. Purdue Univ. Bull. 176,* 1914). Wheat suffers the greatest injury, for the garlic bulbils are harvested with the wheat seeds.

Badly infested wheat cannot usually be sold and is fed to the stock. In the milling of garlicky wheat the garlic bulblets gum the rollers and the buhrstones and interfere seriously with the grinding of the flour . . . the garlic flavor is imparted to the flour as well as to the food made from it. . . . In pastures where garlic is a veritable pest to the dairy farmer, milk of cows fed on garlic has an unpleasant flavor, also the butter made from it (Pipal).

Many attempts have of course been made to find methods of eradication. The high reproductive capacity of the wild garlic, the high resistance against chemical agents of the thin cylindrical leaves with their waxy surface, the ability of the hard-shelled bulbs to stay dormant for several years and to endure periods of drought or cold, the formation of secondary bulblets by each aerial bulbil—all make the problem of eradication a most difficult one. Where the infection is slight, as in lawns, pulling by hand may be effective. It has been recommended (Tinney) that spring crops be rotated over a period of six years, thus destroying the plants in the middle of the growing season each year. It is also claimed that late fall plowing will delay the production of hard-shelled bulbs, which can finally be prevented from forming by the spring cultivation. This method has already met with great success (Scott, 1944). Since the reproduction by seeds or by aerial bulbils is the most efficient method, any way by which the immature heads could be removed would be of the greatest importance. Unfortunately, no controlled large-scale experiments have been made in this direction until recently. Thus, the wild garlic will remain an obnoxious weed in the eastern and the middle United States for years to come, as it has been for more than two centuries.

There are still several unsolved problems in regard to the wild garlic that are worth investigating. It would be interesting to find out whether there are cytogenetic differences corresponding to the morphological and anatomical characters of the different forms. Although it is clear how, by its excellent equipment for the struggle for life, the wild garlic conquered the country into which it came as an immigrant, it is not known why the same equipment did not have the same effect in its old home, And, of course, the discovery of an efficient method of eradicating the wild garlic as a weed would be of great value.

THE BASIC CONSERVATION PROBLEM

LAURA THOMPSON

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VIEWED as a whole, the basic conservation problem in any community involves all of life, human groups and their cultures as well as the fauna and flora, in the total environmental setting. Local conservation programs, if they are to be practical and economical, must not ignore the active and potent human factor. Indeed, to be effective, from the long-range viewpoint, conservation programs should aim at fostering the balanced organization and health of the total community, the human population as well as the plants and lower animals, in relation to the natural resources.

ISLAND COMMUNITIES

It is well known that isolated natural communities tend in time to develop a delicately balanced ecological organization wherein, through a process of symbiosis and accommodation, the various species of flora and fauna attain a mutually advantageous and self-regulating adjustment within the total environment. Ordinarily, we think of the life-web process mainly in terms of plant and animal species in relation to the soil, microorganisms, water supply, and other natural resources. But in remote, isolated areas, such as many island communities, where natural processes have proceeded for centuries undisturbed, it is clear that all of life is involved in the self-regulating adjustment. Seen whole, the total island population of plants, animals, and human beings is a complex, interdependent community, each factor related to the other and to the land base, fishing grounds, soil, fresh water, minerals, and other resources.

Moreover, if we study traditional island cultures from the conservation viewpoint, we find a tendency to organize and integrate the habits of feeling, thought, and behavior of the social group with the world of nature in a way that, in the long run, fosters rather than destroys the web of life. Native conservation measures are often so diffused throughout the culture that their total, cumulative, life-preserving, and life-promoting functions are overlooked by outsiders, even anthropologists. Nevertheless, these measures are present and are often highly effective.

In functioning South Seas cultures, for example, first-fruits taboos, whereby the entire crop of breadfruit, mangoes, or other important native foods is protected until it matures, prevent vital means of subsistence from being consumed before they are properly ripened. This custom not only increases the size of the total crop and provides a mechanism whereby a surplus may be set aside against time of scarcity, but it also improves the native diet by making available the full food value of the mature crop. Indigenous fishing regulations organize offshore and deep-sea fishing activities to take advantage of the weather, the seasons, and the habits of various edible species, as well as to protect local fishing grounds from overfishing and undue disturbance. The development of the men's house complex, correlated with certain taboos on sexual intercourse (as, for example, during the nursing period after childbirth), functions as a means of birth-spacing, population control, and the protection of infant and maternal health. And, as a final illustration, the use of fresh water in functioning island cultures ordinarily does not exceed the annual supply. Indeed, on limestone islands, such as Lau in Fiji, where the natives have access to practically no fresh water except the rain water they are able to catch, a culture has developed in which fresh water plays a negligible part. The islanders drink coconut milk, boil their food in coconut cream or steam it in the earth oven, and bathe in the sea. Fresh-water problems arose when the missionaries introduced a new concept of modesty requiring the wearing of lava-lavas made of cloth. Today, on some islands, almost all the available water, both fresh and brackish, has to be used for laundering.

Space limitations forbid further development of this point, but the evidence points to the conclusion that isolated human groups, through their cultures, tend in the long run to cooperate with natural processes and to foster rather than destroy the web of life. In other words, the culture process tends through time to complement and reinforce the basic nature process.

This conclusion is of utmost significance to the total conservation problem and to human welfare

in general, for it means that ecological communities are composed of human beings, as well as other animals and plants, within the total environmental setting, with human beings traditionally playing an *active and positive* part in the multidimensional attaining and maintaining of a balanced, healthy adjustment. Man, through his culture, tends to cooperate with nature in a mutually dependent, life-promoting endeavor. This means that the basic conservation problem is not primarily one of how this topsoil may be saved, or that species of bird, tree, or animal may be protected from extinction, or of which forests, watersheds, or fisheries should be made into reserves, or of how a unique biotic equation or archeological monument may be preserved for posterity. Rather, the basic conservation problem is that of using and adapting indigenous attitudes, roles, and institutions, supplemented where necessary by appropriate new ones, to the end that the resident groups of each area, with the advice and help of scientists and administrators, may foster the emergence of balanced, healthy total communities.

It is necessary to stress the human factor in the conservation problem because there is a strong tendency among scientists and laymen alike to think of conservation mainly in terms of birds and forests, soil, and water supplies, rather than in terms of a balanced relationship between local populations and their natural environments. Man is not only a major factor in the life web; he is the *only* agent whereby a conservation program for a local area may be actively implemented.

There is a very real danger that a one-sided or limited conservation blueprint, drawn up by highly specialized technicians outside the area under consideration, may inflict a great hardship on the indigenous populations to the extent that it is made effective. If, for example, in order to preserve a near-extinct species of bird or a certain biotic arrangement, we forbid the people access to relatively large areas of their privately or communally owned lands (as is implicit in recommendations of the Pacific Science Board), we may be denying them vital resources from forests, uplands, streams, fisheries, or garden lands on which the subtle balance of their economy has long depended. If, furthermore, we appropriate such resources without valid compensation, as is often done (for example, with the entire output of phosphates on Angaur Island, the natives' only nonrenewable cash asset, now being shipped to Japan), we violate every tenet of just and economic government administration.

Manifestly, the only practical and economical approach to a coordinated, long-range conservation program is to enlist the interests and efforts of the local groups themselves. If there is to be an effective conservation program, it is primarily the people who will have to organize and carry it out. The indigenous populations are, as a rule, much more vitally and personally concerned than scientists or administrators in conserving local values—whether they be birds, fish, trees, or garden lands. From the practical standpoint, they are the ones fundamentally involved in a local conservation program, for their welfare, their very existence, depend on it. They have proved in the past, and can now and in the future prove, their capacity to devise practical and effective means to implement such programs.

But they need help. Modern scientific discoveries and technologies, hand in hand with alien, exploitative ideologies, purposes, and habits, have to a large extent created the conservation problem. The destructive processes thus initiated have now proceeded so far that they are seriously interfering with the recuperative powers of nature. Unless the resources of modern science are applied to the problem, the development or restoration of community balances may be indefinitely delayed. It is too late for *laissez faire*. To ensure the emergence of new, healthy adjustments between nature and man, we must foster the curative processes of nature through the methods of the social, biological, and physical sciences.

MICRONESIA, A TYPICAL ISLAND CULTURE

The problem of conservation in Micronesia, currently under discussion by the Pacific Science Board of the National Research Council, is of utmost concern to all those interested in the welfare of such island cultures. Our Trust Territory is composed of hundreds of small islands, each with a unique web of life, which developed its obscure and subtle interrelationships through the centuries when these islands were relatively undisturbed. During World War II and the postwar period the sudden impact of the outside world seriously disturbed the insular ecological balances. Destructive faunal, floral, bacteriological, and chemical agents were introduced; irreplaceable topsoil was bulldozed off and dumped into the sea to make room for military installations; ground-water levels have been dangerously lowered; and coconut, mango, and other fruit trees upon which the natives depended for subsistence, as well as many game birds and animals, have been killed. In addition, resources indispensable to the native economy—such as garden land, fisheries, fresh-water sources, and phosphate deposits—have been withdrawn indefinitely from native use.

On Guam alone, for example, almost one third of the island acreage (which totals only about 225 square miles), including much of the best farm land, has been requisitioned from native ownership since 1941. Inasmuch as another third of the island had already been appropriated by the United States government, this means that today only about one third of the island remains in native hands, although land traditionally has been and still is the basic and most valued natural resource.

These disturbances are the more devastating since, compared with continental areas, the life webs of island communities are highly vulnerable. In oceanic islands ecological organizations consist of unusually small populations confined to limited areas, and they are delicately balanced, highly endemic, easily disturbed, and susceptible to quick extermination. Indeed, the loss or addition of a single species may seriously jeopardize the whole organization. For example, the introduction by the Japanese of a giant snail as a table delicacy upset the balance on several islands.

Since the island life webs evolved in the absence of large land animals, they have little or no resistance to the ravages of cattle, sheep, goats, and deer. On the island of Tinian, for instance, a species of deer brought in by the Spaniards soon ran wild, increased manyfold and destroyed much of the native vegetation. The introduction of cattle and deer, which the Chamorros hunted by the brush-burning method, is largely responsible for the fact that about one third of Guam, the south-central region, is heavily eroded. Most of this area is now not only unfit for agriculture, but also is poor grazing range.

This brings us to an important point. In each of the examples cited (and in hundreds of others that might be mentioned), it is man who has been responsible, either directly or indirectly, for the change in island biotics. In remote, seabound areas it is inescapably clear that all of life is involved in the ecology of the island. Hence, if we are to understand the total conservation problem in Micronesia, human societies may not be left out of the picture any more than any other relevant factor, such as phosphates or fishing, may be omitted.

Whereas plant and animal species tend to adjust to the island web of life by obscure processes of symbiosis and accommodation, man's role is more active. Indeed, it is clear that in such situations man's part has been not only active but also irresponsible and destructive. It is important to note, however, that in each case mentioned we are witnessing the aftereffects of relatively recent activities on the part of intrusive, nonindigenous peoples. When we examine the evidence regarding the traditional role of the indigenous populations in island ecological processes, we get quite a different picture.

Manifestly, the only practical and economical approach to a coordinated, long-range conservation program in Micronesia is the enlisting of native initiative and effort. The natives are not interested in the preservation of "living museums" or "laboratories for the study of evolution." They are interested in rehabilitating themselves and their islands from the ravages and consequences of total war. If there is to be an effective conservation program, it is primarily the natives who will have to organize and carry it out, for on many of the islands all the residents are natives, the only contact with the administering authority being short official visits by naval administrators, often months apart.

If the Pacific Science Board and the Island Government do not meet this issue, they will be missing a golden opportunity to lay the firm foundations for a model trusteeship administration in Micronesia, one that is practical and economical as well. On account of the vastness and remoteness of the region, and its historical and cultural diversity, Micronesia presents one of the most difficult problems in dependency administration in the world. Without sound long-range goals, native-oriented and native-implemented, and without the help of technicians experienced in the methods of democratic, integrative leadership, we may pour tens of millions of dollars into the area with little positive result either in terms of improved native welfare or of conservation of resources. But a scientifically based and coordinated conservation program, organized and developed by the native groups themselves and integrated with their own attitudes, mores, and institutions could, with wise, democratic guidance, effectively supplement the curative processes of nature and foster the re-emergence of balanced, healthy island communities. The administrative organization needed for this island trusteeship task—an organization scientifically informed and motivated and democratically oriented—will be difficult to attain under whatever auspices. Under the present naval government or any other setup dominated by the military, it cannot be attained. The facts suggest, therefore, that from the viewpoint of island health and welfare and administrative efficiency, the announced policy of President Truman to transfer jurisdiction of the Pacific islands to a civilian agency under carefully framed organic acts urgently needs to be implemented.

THE EXPERIMENTAL METHOD IN THE STUDY OF HUMAN RELATIONS

F. STUART CHAPIN

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IN 1916 I wrote an article on this subject which was published in THE SCIENTIFIC MONTHLY of February–March 1917. It may be of interest, after thirty years, to review briefly the development of experimental designs applied to the study of human relations.

The experimental study of human relations in the free community can hardly be reduced to laboratory conditions of control. Hence the present article will exclude all experimental studies conducted in an artificial or classroom situation and confine attention to six illustrative studies that were made in the free community situation. This limitation has the advantages of distinguishing sharply, first, the fundamental principle that controlled observation relies on the matching of measurements rather than on physical manipulation; and, second, the kinds of practical obstacles encountered in all attempts at controlled observation in the free community situation. The six studies to be described are all published and show how systematic efforts were made to control variable factors by matching on measurements between an experimental group that received some social program and a control group denied this program.

In the interest of clear thinking about this problem it is helpful to distinguish, first, the trial-and-error "experiments" of social legislation as a means to achieve some desired end (public housing as a means to improve the adjustment of low-income families); second, the operations of natural social forces that produce an effect; and, third, the use of experimental designs as a method of study of the first two, in order to determine the degree of success in the attainment of a desired social end, or to measure the effect of some social force. Since a social reform program is a social force that operates according to some plan of action, our description will be confined to illustrations of this, the first of the foregoing distinctions, rather than illustrations of the use of experimental designs in the study of unplanned consequences of combinations of independently planned social actions, which is the second type distinguished above. This second type would take us too far afield, since it concerns the phenomena of conflict among the means-ends scheme of special interest groups, a resulting composition of social forces, and the emergence of unplanned consequences which plague the authors and the leaders of the component but independently planned programs. Business cycles, inflation, mass movements, etc. are examples of this second type. Such mass behavior can hardly be studied experimentally until we have solved some of the problems connected with experimental study of their simpler component means-ends relations. Hence the limitation noted above.

Five methods of observation and analysis are essential in studies of human relations by experimental designs: social measurements by use of standardized psychometric or sociometric scales; random samples or a stratified random sample; control of observations by matching on social measurements; use of the null hypothesis; and qualified application of the principle of probability.

Leaders of social reform have often found that the ends they desired to achieve by means of legislation are seldom fully attained, usually because there are unexpected results which flow from the unplanned combinations of other independently planned social actions. In the free community situation of a democratic social order, it is customary to protect the rights of minority groups (political, religious, and social) in freedom of assemblage, freedom of speech, freedom of religious worship, etc. The consequences of these democratic freedoms are that many special-interest groups in society set up their own means-ends schema and work at cross-purposes to other minorities to achieve their contradictory goals. These phenomena take the familiar forms of social tensions, group conflicts, achievement of one at the expense of the frustration of the other, etc., all of which appear to the foreign observer as evidences of internal confusion and mislead such observers about

the existence of a deep current of agreement on essentials. This web of human relations fluctuates within certain limits of social elasticity, now seemingly stretched to the breaking point, and then contracting again to a more stable social structure. How can one ever untangle the separate strands of this complex social fabric? Although adequate answers to this question await the study by experimental designs, or by other methods of analysis, of the involved chain of causes and effects now hidden in the unplanned consequences that arise out of combinations of independently planned social actions, it can be shown that a real beginning has been made at a simpler level in the study of particular cause-and-effect sequences which are called the means-ends schema of social reform. The beginning is real because it is based upon concrete studies of social facts and not on armchair social philosophy; it is promising because the method of experimental design has now been used with moderate success in tests of such problems as direct relief versus work relief, public housing, and juvenile delinquency; it is objective because the method is susceptible of repetition by equally competent observers who use quantitative descriptions of human relations.

Despite some modest development in experimental designs, my present concern is with the still-unsolved problems of the method, so that the difficulties to be overcome may not be lost sight of in an account of some undoubted achievements.

There are three general patterns of experimental design in the study of human relations in the free community situation: first, a cross-sectional design in which comparison is made for a given date between an experimental group which receives a social program, and a matched control group denied this program; second, a projected design in which before and after measurements are made upon an experimental group which received a program over an interval of time, and a matched control group denied this program; and, third, what may be called the ex post facto design, in which a present situation is taken as an effect of some assumed and previously operating causal complex of factors, and, depending upon the adequacy of accessible records, an experimental group and a matched control group are traced back to an earlier date when the force to be measured began functioning upon the experimental group but not upon the control group.

Cross-sectional experimental design. Nathan Mandel used this method in a study of the Boy Scout program and the measured personal adjustment of boys. It is at once evident that some measures of personal adjustment that are reliable and valid will be required, and also some measures of other factors to be controlled, in order that the real association between the Scout program (the means, or causal variable) and the personal adjustment (the end, or effect variable) may be described. The three measures of adjustment used were the Bell *Adjustment Inventory*, Rundquist and Sletto's *Morale* and *General Adjustment* scales (all measures of how the individual feels), and the Chapin *Social Participation Scale* (a measure of overt activity in organized groups).

Choosing every tenth case in the file of 2,050 Boy Scouts in the 1934 "drop-out" file of the Minneapolis area yielded a total of 205 boys. Only 102 of these were found and interviewed in 1938. Since 103 had moved away, could not be located, would not cooperate, or were deceased, the residual sample ceased to be random. The remaining 102 cases were divided into two groups, an experimental group whose training had lasted 4 years on the average, and a control group with an average tenure of 1.3 years. When these were matched on four available traits in the record (birthplace, urban or rural, father's occupation, health rating, and school age-grade ratio), there was a further shrinkage of 22 cases, so that the final totals were 40 in each group.

It was found that the 4-year Scouts were slightly better adjusted on both the Bell and the Rundquist-Sletto scales than the control group of drop-outs. Interpretation of these results as indicative of mere conformity and conventionality rather than of somewhat better integration of personality would then have to explain why it was that the drop-outs showed higher scores on the Chapin *Social Participation Scale* than did the 4-year Scouts, since a higher social participation score is evidence against the rejection from membership of the drop-outs by the organized groups in which they were active. All in all, this study was somewhat inconclusive, since the advantage evidenced by higher adjustment scores of the 4-year Scouts was too slight to provide local proof of the hypothesis upon which the Scout program is based; but the results were at least consistent with the expectations implicit in the Boy Scout program.

During the great depression of the 1930s it became evident that material relief alone was a mere stop-gap, and that self-respecting assistance to the unemployed should take the form of a work-relief program. Hence the Works Progress Administration was undertaken. It was claimed that the WPA would develop morale and maintain it

more effectively than a direct-relief program of material assistance.

To test the validity of this claim an experimental design study was made in St. Paul, Minnesota, in 1939. A total of 465 cases were on direct relief in March of that year, and 8,074 persons were on WPA. A 5 percent random sample of these WPA cases, or 412 persons, was taken as an experimental group to be compared with the 465 relief clients. To make these groups sufficiently homogeneous for valid comparison, seven conditions were set: living in St. Paul; working in Ramsey County; not previously on WPA; and not single, widowed, separated, or divorced. Meeting these conditions reduced the WPA experimental group to 324 cases, and the control group of relief clients to 198. These 522 cases were interviewed in April–May and the total still further reduced by 320 cases because of refusals, could not be located, had moved away, deceased, sickness, gainfully employed, changed status, etc. As a result there remained 130 cases in the experimental group of WPA and only 72 direct-relief clients. These two groups were then matched on seven factors: age, sex, race, nativity, years of formal education, usual occupation, and size of family. Had this matching on traits not been done the obtained variations in measured morale and adjustment might have been due to one or more of these factors, rather than associated with being on WPA or on direct relief, the association we set out to measure. Again, both groups lost cases because unable to match, so that the terminal groups consisted of an experimental group of 80 WPA matched on seven factors against a control group of 42 on direct relief.

During the interviews every individual was measured for his morale and his general adjustment score on the Rundquist-Sletto scales, and for social participation and social status on the Chapin scales. On each of these scales the WPA group showed better average measures of adjustment than did the relief group. Differences between these average group scores were, however, not statistically significant, so that chance could account for the differences. But the differences were in the expected direction, and a measure of all four in combination as a pattern of response to differential treatment by the two contrasting programs showed that these differences in pattern could occur by chance somewhere between 1 in 10 and 1 in 50. Again, our experimental design study failed to yield positive proof of a hypothesis of high association, although it did yield results not contrary to this hypothesis.

To summarize the most significant findings from these two examples of cross-sectional experimental design, it may be noted:

1. That no test of cause and effect or of concomitant variation is possible by this method; the only evidence obtained is that of an *association* at a given date between two factors (different programs of assistance on the one hand, and measured adjustment on the other), but this association was in the expected direction, if it is assumed that WPA is superior to direct relief in attaining better adjustment.
2. The enormous losses of cases owing to selective conditions in the natural community, plus losses from matching, destroyed any randomness of the original samples, and increased the magnitude of the standard errors, thus reducing the statistical significance of any differences obtained.
3. Since the residual groups departed from randomness, no generalization can be made from such studies to any larger universe or universes from which the groups were taken, although it remains true that conclusions for the limited groups studied do have a valid basis in fact.

Projected experimental design. Unless social cause and effect can be subsumed in the area of human relations, there is little hope of a rational explanation of the vexing problems of the social order. We have just shown that the cross-sectional experimental design, because it is limited to a controlled comparison of two groups at a given time, fails to disclose cause-and-effect relations, although it may be suggestive of hypotheses for study by more elaborate methods. Does the projected experimental design offer a more hopeful promise of discovery of social cause and effect? Two published studies will now be analyzed in an effort to answer this question.

The first study was an attempt to measure the effects of a public low-cost housing project upon the social adjustment of slum families in Minneapolis from 1939 to 1940. One year is admittedly too short an interval for any real test, but practical considerations set this time limit, and so we are obliged to make the best of it.

This study began in the spring of 1939 when 108 former slum families having low incomes had taken up residence in Sumner Field Homes, a PWA housing project planned in 1935. The control group consisted of 131 other families residing in the same slum dwelling during the period. Although these 131 families were initially so much like those eligible and admitted to residence in the project as to be borderline or deferred cases who might be admitted later, an attempt was made to make the experimental group of residents and the control group of slum dwellers still more alike by matching on ten control factors: race or cultural class of husband, of wife; occupational class of

husband, of wife; employment status of husband, of wife; number of persons in the family; income of the family; and years of formal education of wife. As in the studies previously reported, many cases were lost by inability to match. In all, 59 cases dropped out for this reason—47 from the experimental group and 12 from the control group. Because of the lapse of time between initial and terminal measurements, various events intruded and 50 more cases dropped out for these reasons—12 from the experimental group, and 38 from the control group. Forty-eight more cases were lost before the initial interviews were completed, owing to mobility, refusals, and other reasons, of which 5 were from the experimental group and 43 from the control. Thus the final matched groups (frequency distributions equated) consisted of 82 persons, 44 in the experimental group and 38 in the control group.

This test of low-rent public housing as a means to achieve the end of improved adjustment of slum families, or as a measure of a program of social reform as a cause, to produce improved adjustment as the effect, as the case may be regarded, relied upon changes in measured social adjustment before (1939) and after (1940) the program had operated for one year, as evidence of proof. The measures of social adjustment used were the same as those applied in the WPA–relief study described above. Since no changes in measured morale or in general adjustment were found statistically significant in a comparison of average scores of each group before and after, and also because each family had resided in the *same* dwelling unit for the period, there were in fact thus added three additional controls to the ten noted above, making in all, control on thirteen factors.

When, however, attention was directed to the changes that occurred in social activities (social participation scores), in the percentage use-crowded, and in the condition of the living room of each home (measured by the Chapin social status scale), it was found that a pattern of response consisting of these three factors in combination showed a difference between change in the experimental group and change in the control group that was of high statistical significance. Whereas the pattern of change on these three factors in the control group was not statistically significant (multiple critical ratio, 1.82), the corresponding change for the experimental group was highly significant (multiple critical ratio, 6.01); and the difference between the changes of the two groups on this measure was also highly significant (multiple critical ratio, 4.97). Such high critical ratios as these are so unlikely to occur in chance that a cause-and-effect relation may be inferred. In a certain sense this is a gratuitous conclusion because the public-housing program was intended to interfere with chance and to create for the project residents improved housing, so that it might be more surprising if it did not effect improved adjustment. This desired effect cannot be established by mere wishful thinking, however, but only by the evidence of measurements on scales that had been standardized to measure adjustment prior to the existence of the change they were used to test. Furthermore, although science often merely confirms the results of practices based on experience, it is by no means certain that a scientific test will not disprove the effectiveness of popular practices; hence the justification for the present study.

The real question that faces us in making inferences from the facts obtained by this experimental design is: Did housing per se cause these changes in pattern of response? Only a positive answer to this question, phrased in terms of a low probability of chance as an explanation, would prove the effectiveness of the public-housing program in this example, and then only if all unknown factors had been controlled. Although thirteen factors were controlled in rough degree, the remaining known but unmeasured factors (health, for example), and an undetermined number of unknown factors, were not controlled. Thus the results of this test by experimental design offer no final conclusion. Furthermore, since the groups compared were nonrandom samples, no reliance upon probability tests is permissible as a basis of generalization to any larger universe of similar "experiments" by public-housing authorities.

Where does this leave us? On the negative side we may offer the opinion that the results are sufficiently suggestive to urge repetition of this type of study on similar cases using like methods of research in the hope that replication will yield corroboration of the results. For only by replication in numerous similar studies may we escape from the dilemma of whether the obtained significant differences were due to the nonrandomness of the samples, or to the fact that they were drawn from different universes (i.e., universes that were made different by virtue of the public-housing program, the objective that was to be tested by the experimental design). Should the same results be found on many trials, then generalization from even nonrandom samples to a universe might be valid and justified. On the positive side we may say that the differences found were of an absolute magnitude which would be regarded as highly significant if

they were found between two random samples.

Of the five principles enumerated as essential to study by experimental designs, three have been illustrated: social measurements by use of psychometric or sociometric scales; control by matching on measurements rather than by physical manipulation (the conventional and mistaken idea about control of variable factors); and qualified application of the principle of probability. But the housing study illustrates also the advantages to the research student that follow from the use of a null hypothesis in sociological research wherein the purpose is to test the results of a social reform program.

The conventional working hypothesis is a positive assertion: "Public housing improves the social adjustment of individuals and families living in a slum." There are at least two difficulties to be overcome in efforts to test such a hypothesis: first, the statement uses the normative term "improves," which implies the question "What is improvement?" Whose standards of what is improvement are to be taken? Shall we rely on the judgment of the housing manager, or on the opinions of those who promoted the project, or on the critics of all public housing? Obviously, any such definitions are open to subjective considerations which stem from different desires. This problem is simplified when standardized sociometric scales are used, since there was incorporated into the initial construction and testing of such scales elements of objectivity not present in individual opinions. Moreover, the norms of such scales were discovered in previous studies, so that the application of these scales in any present study is not affected by any desire to vindicate or to disprove the program being investigated. A second difficulty is more serious; it consists in the fact that "improvement" is an open-ended concept. How much change in the desired direction is improvement? When the positive hypothesis is replaced by a null hypothesis, this difficulty is avoided.

Three null hypotheses susceptible of proof or disproof by facts may be set up as follows:

1. When measures of adjustment are made upon an experimental group which receives a social program and an experimental group denied this program during an interval, and the two groups are matched on a number of factors, there are no changes in measured adjustment during the interval. Since the facts obtained in the housing study show the existence of changes, this null hypothesis is proved false.

2. If changes in measured adjustment are found in these groups, the changes are not statistically significant. Again this null hypothesis is proved false by the results found.

3. Although changes may be found, and these changes are statistically significant, the difference between changes of the two groups is not statistically significant. This null hypothesis is also proved false by the factual evidence.

It is evident that it is a much simpler task to prove that differences do exist, that these differences are statistically significant, and that the difference in changes is statistically significant as departures from zero than it is to prove that the changes are in the nature of improvement; and this is particularly the case in studies of problems of human relations, wherein bias is hard to avoid and the subject matter of study is emotionally disturbing to the observer because it involves his personal value systems.

A second study using the projected experimental design to test the effectiveness of a social program of differential treatment is that reported by Harry Shulman in New York City. This is a study of the effects of treatment to prevent juvenile delinquency by a controlled-activity program. The program consisted of workshop and game-room activities, classes in creative art, woodwork, leather, and metalworking, which met three sessions a week for two hours a session over a period of three successive school semesters. Groups of 50 problem boys and 80 normals were mingled naturally in these activities. The problem boys included chronic truants, incorrigibles, serious personality problem cases, and some charged with arson and theft. The normals were nonproblem cases obtained by serial selection from class-roll books of children who had never dropped below a B grade on their studies. The experimental group to receive the program consisted of 65 boys, including 25 of the problem cases and 40 normals. The control group was similarly constituted but denied the program. At the beginning there were 310 boys, 155 in each group, who were ten and one-half to fourteen and one-half years of age and in Grades 4-A to 8-A from four public schools in socially substandard areas of New York City.

The measure of community adjustment used consisted of 13 of the 66 Baker-Traphagen items which measured behavior status. Comparison of the results for the problem boys of the experimental group with the problem boys of the control showed that 72 percent of the problem boys in the experimental group which received the program improved, and only 33 percent of the problem boys in the control group gained. Meanwhile 28 percent of the former had lost, and 66 percent of the latter. In tests before and after in the classroom situation all differences among the mild-to-medium conduct-disorder cases were statistically significant on scores of the Haggerty-Wickman-Olson behavior rating scale for the two problem children groups

Since these favorable results might have accrued from differential changes in home environment during the experimental period, a case study was made of factors of family disorganization (broken home, marital disharmony, public assistance, economic maladjustment, children's illness, mental deficiency and disease, unethical or antisocial example, etc.), of defective social relationships (between parents, parent-child, and community), and also of improper discipline. The results of this analysis showed no appreciable differences in changes for the problem children of the two contrasting groups. Thus the intrusion of environmental factors of the surrounding community had not operated to confuse the relationship between the treatment program as a cause and the diminution of behavior problems as an effect.

Although the two studies in projected experimental design just described illustrate some advance toward the scientific goal of discovery of social cause-and-effect relationships, each study stands by itself as a closed system, and no scientific generalization may be made from either to the larger universe of housing or juvenile delinquency. Again, repetition by similar studies on like subjects, using the same methods of research, is the only avenue of approach to a reliable basis of generalization.

Ex post facto experimental design. For many years students of human relations have sought a valid method to discover cause-and-effect relations which it is believed may be hidden in the records of past social events. The ex post facto experimental design was developed as one attempt to clarify this methodological problem.

A study that illustrates the use of this ex post facto method was made by Helen Christiansen under my direction in 1935–38. The positive hypothesis to be tested was the longer the period of high-school education before leaving school, the better the subsequent economic adjustment in the community. Here the formal high-school course of instruction is taken as the program variable, or the causal factor, and measured economic adjustment is taken as the effect variable. To test this hypothesis the records of 2,127 high-school students who left school nine years earlier (1926) were obtained and analyzed. In that year 1,130 had graduated from the 4-year course in all high schools of St. Paul, Minnesota, and 997 had dropped out of school, having been in 1926 at the end of their first, second, or third year of study. The graduates were taken as the experimental group and the drop-outs as the control group. The measure of economic adjustment in the community situation chosen as the dependent variable was the percentage of shifts on jobs from 1926 to 1935 which involved increase in salary, no change in salary, and decrease in salary. This is admittedly a crude measure, but the data could be obtained by interviews of all persons of the original 2,127 who could still be located in 1935.

Considering the relatively long experimental period of nine years, substantial losses of cases were to be expected. As a matter of fact, 933 individuals were lost for analysis, 459 of the graduates and 474 of the drop-outs, for reasons of death, moving away, not located, or incomplete records. After matching on six control factors, age, sex, father's occupation, parental nativity, neighborhood of residence, and average high-school marks (no I.Q. data were available in 1926), the two groups were reduced to 145 individuals each.

Analysis of economic adjustment to the community in 1935 showed a regular decline in percentage with decrease in salary or no change in salary, with each additional year of high-school education for drop-outs of 1, 2, and 3 years of study, and a regular increase in percentage with salary increases. The 4-year graduates attained the highest percentage with salary increase for 1926–35 of any group, and their percentages of cases with decline in salary or no change were approximately the same as those of the 3-year drop-outs. When the matching technique was made more precise—that is, by identical individual matching, instead of equating frequency distributions on each control factor—the two groups were reduced to 23 cases each. The trend in percentages on the salary criterion became even more pronounced, and the advantage of the graduates over the drop-outs was much greater. All in all, the positive hypothesis seems to have been established for this study, provided the results can be explained as not due to health factors uncontrolled, the factor of persistence in school work, the existence in the group of 933 lost from study of trends contrary to those found in the groups that were analyzed, or unknown factors. Again no decisive proof has been achieved; only the probability of a cause-and-effect relation between length of school attendance and subsequent economic adjustment in the community.

But this matter of probability deserves further exploration. To test the statistical significance of the percentage differences found between the experimental and control groups, two random samples of 23 cases each were chosen from the larger experimental and control groups, but the individuals in these random samples were not matched (as were the individuals in the 23-case terminal

experimental and control groups which showed the largest differentials). Analysis of the random samples showed indicia of differences in community adjustment occurring by chance as 1 in 14, as compared with 1 in 200 for the 23-case matched samples. Since the original 2,127 cases constituted the universe of all who left high school in 1926, and the remaining 1,194 cases composed the remainder after losses of 933 cases, both the random samples and the experimental groups were from a subuniverse, so that comparison of real random samples with nonrandom groups introduces in this experiment a more adequate basis for the use of probability tests than was the case in any of the other studies described in this article.

In summary, it may be said that the differentials in favor of the graduates seem too large to be explained as mere chance fluctuations, and of absolute magnitudes that cast some doubt upon the validity of the four alternative explanations, which would set up the claim that it was not the length of high-school education that caused better economic adjustment in the community of graduates. The comparison with the random samples yields results also consistent with the foregoing inference.

But, again, we must caution against generalization to all high-school programs of all areas of the United States. Perhaps the place and time studied were unique. On the other hand, it may be pointed out that the 1926–35 period included the depth of the great depression of the 1930s, so that the hypothesis was substantiated for an interval of sharp testing by unemployment, and hence that our conclusions have a safety factor that makes them conservative.

A variation in the ex post facto design was made in a study of public housing and juvenile delinquency in New Haven by Naomi Barer from records of 1944 on 317 families traced back to 1924. She made a self-comparison of a group of 649 children seven to seventeen years of age in these families, for the period 1940–44 to the period 1924–40, when the same families and their children lived elsewhere. From 1924 to 1940 the rate of juvenile delinquency per 100 children per year was 3.18; when the same subjects were residents of the housing project the rate had declined to 1.64. This is a statistically significant decline. During the years 1940–41 there was an increase of 9.1 percent in total juvenile delinquency in the city of New Haven over that of the period 1927–40, so that no general decline in juvenile delinquency occurred in the community at large, which, if it had happened, might explain the decline of the experimental group of children in the housing project. No generalization is justified from this study alone about housing and juvenile delinquency at large. Only corroboration from repetition of the experiment will furnish proof of a hypothesis that good housing operates to reduce juvenile delinquency.

Two types of critics deplore the kind of studies herein described. There are the "practical" reformers who have expressed the opinion that this kind of detailed analysis is superfluous since it is mere common sense to expect to find improvements following upon well-matured plans for social betterment. But are desire and wishful thinking the sort of support upon which to base and to justify expensive and elaborate social programs? Then there are sincere critics who express the judgment that the time and expense of detailed study by experimental designs might better have been applied to the investigation of problems wherein the expectation of significant results had a firmer basis in technical and precise research methods. No adequate answer can be made to this criticism. The reader will have to decide for himself whether exploration by these methods of experimental study, admittedly crude and lacking in precision, is worth the time and effort put forth. In any event, I have found these experimental designs interesting, and sometimes exciting, ventures. They are offered for what they may be worth, and their limitations have been stated and reiterated throughout this article.

Three closing comments may be made. First, objective evidence has been offered to show that experimental designs may be applied to the measurement of problems of human relations with some expectation of a partial clarification of social cause and effect in society. Second, the chief obstacle to the experimental method in the study of problems of human relations is removed, since it stemmed from a false analogy between "social experiments" that shove people around and laboratory experimentation that manipulates physical matter, whereas the fundamental principle involved is observation of social relations under conditions of control, which conditions are attained by matching on social measurements, and do not require interference with personal freedoms. Third, the methods of experimental design offer the promise of an objective procedure for evaluating the effects of some types of social reform programs. Such programs consist of the approved means chosen to attain some desired or valued end. This being the case, it seems possible that further extension of the methods described herein will lead to an exper-

mental test of values, phenomena hitherto circumscribed by desire and wishful thinking and suffused with emotional attitudes.

If the first expectation is realized, we may have in it the beginnings of a rational basis for the amelioration and control of some of the problems of human relations. If the third is realized, we shall have a rational substitute for mere subjective opinions and sanctimonious or self-righteous judgments about what may be done by social action to achieve collective desires. Furthermore, through the replication of experimental design studies, which attempt to measure the effectiveness of specific means-ends schema planned to attain specific goals, it may be possible to develop a systematic mosaic of nonrandom samples that will possess a degree of representativeness to compensate for lack of randomization, and thus to supply a basic representativeness upon which reliable scientific generalization may rest. Finally, if the foregoing development takes place, an approach will have been made to the solution of the most difficult methodological problem of all, to describe objectively the tortuous chain of social cause and effect that now lies hidden in the unplanned social consequences that seem to flow from the combinations of innumerable and independently planned social actions within a democratic society.

SONNET

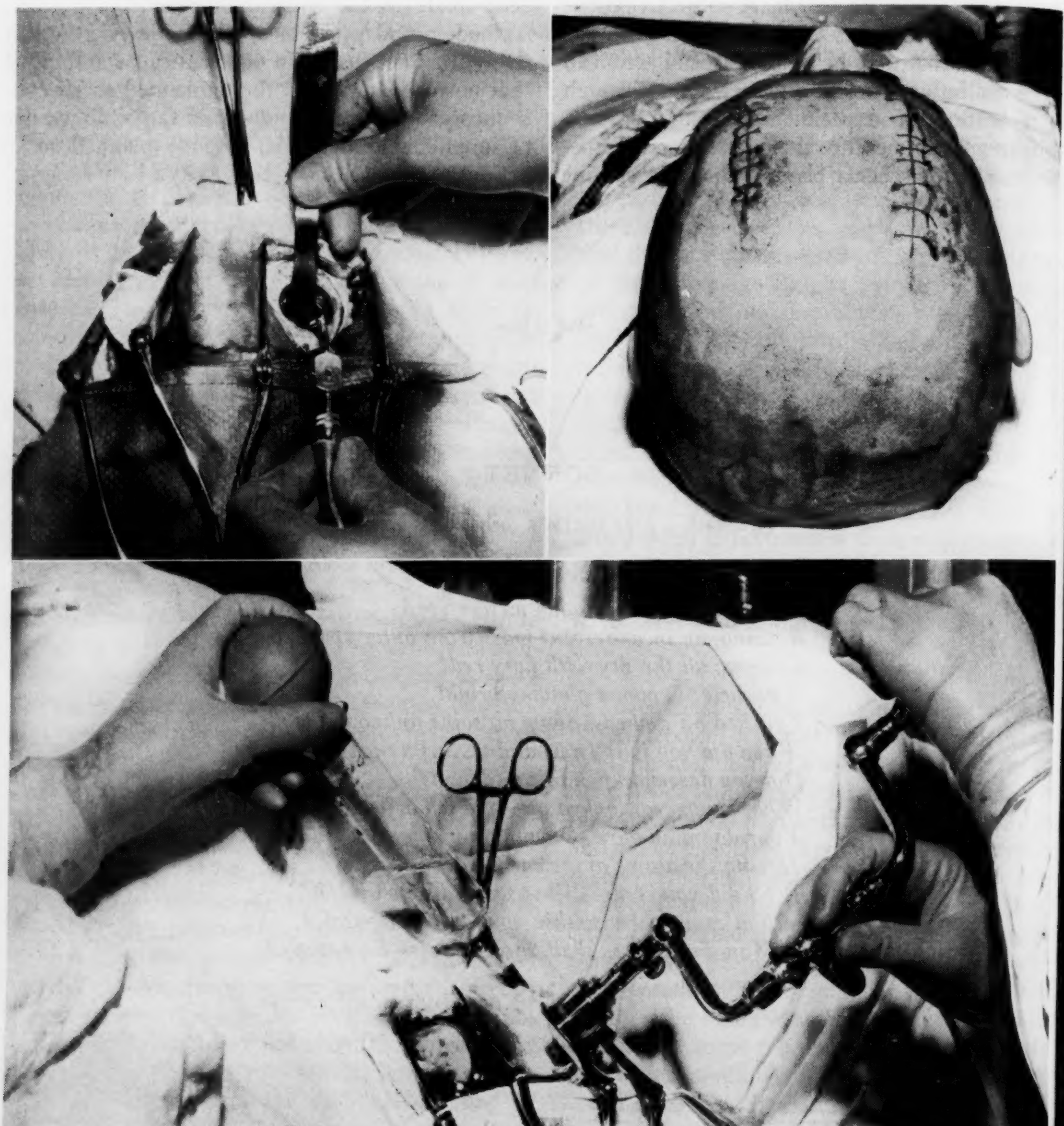
(ON THE GIANT METEOR CRATER IN ARIZONA)

Was it by chance it fell upon this place,
Or was it by design some meteor sped
Roaring, an incandescent mass from outer space,
Coloring all the sky with fiery red?
O miracle! O comet glowing bright,
Touched off and cast down by some unknown hand,
When did you fall? In what dim, awful night
Did you descend upon this arid land?
What conqueror looked up—and fell before
Your dooming blow, O conqueror mightier still?
Jarring the atoms of the earth's deep core,
Why did you come? What word did you fulfill?
Not even when, within your tomb self-sealed,
Man finds you, shall these riddles lie revealed.

NOEL RODERICK PEATTIE

SCIENCE ON THE MARCH

PSYCHOSURGERY—NEW HELP FOR THE MENTALLY ILL



Standard leucotomy by the Lyerly open approach. Photographs by courtesy of Dr. William Beecher Scoville, Hartford, Connecticut.

IN Lisbon, in 1935, a professor of psychiatry by the name of Egas Moniz, together with a neurosurgeon, Almeida Lima, opened up a spectacular new field in the treatment of mental diseases. They operated on the brain of a mental patient.

Brain surgery in itself was not entirely new. By that time brain abscesses and tumors were already being removed successfully, but Moniz and Lima's venture was unprecedented in that they operated on apparently healthy brain tissue in the frontal lobes. Out of their efforts have come the brain operations known today as *prefrontal lobotomy, topectomy, transorbital lobotomy, lobectomy, thalamotomy,* and so on, each aimed at alleviating various types of mental disturbance. These operations have already brought relief to a large number of patients who otherwise would have been doomed to a lifetime of the hopelessness and suffering that have been contained for centuries in chronic mental illnesses.

Because apparently healthy brain tissue is sacrificed in the process, the prefrontal lobotomy and related surgical techniques have been slow of acceptance in many quarters. With the accumulation of experience and the refinement of techniques, however, brain surgery (or *psychosurgery,* as this particular aspect of surgery is called) now has a considerable body of evidence in its favor. Last August 3–7, more than 200 neurosurgeons, neurologists, and psychiatrists, representing twenty-seven nations, gathered in Lisbon for the First International Congress on Psychosurgery, which was held in honor of Dr. Moniz, and was organized primarily by Dr. Walter Freeman, American pioneer in the field. Reports were made on 5,000 cases of psychosurgery—a world-wide scrutiny of the original operations and the techniques that have been evolved since then.

The present-day surgical intervention in the frontal lobe region is based on the prevailing concept of the function of the frontal lobes, of which there are two, and which are very much more prominent in man than in other species. Of all parts of the brain, and probably of the whole body, the frontal lobes have been the most difficult to understand. The frontal lobes have resisted scientific probing, although nearly every other region of the brain had its functions fairly well delineated. There are the areas of the brain that govern speech. This we know through laboratory experiments, and because experience has shown injury to those areas to render a person speechless, although no harm had come to his speech organs. Certain areas have been shown to govern sight; others, hearing, writing, vision, motor ability, and so on. The frontal lobe areas, long the greatest mystery, and studied by anatomists, neurologists, psychiatrists, physiologists, and psychologists, have now been marked, largely on an empirical basis, as the areas of association or integration, governing such high mental functions as judgment, imagination, and initiative.

In the beginning, Moniz theorized from the mounting indications that great cerebral activity is centered in the frontal lobes. Convinced that certain neuropsychiatric conditions reflect a morbid fixation of various frontal association pathways, he reasoned that surgical interruption of cell connections in these areas might relieve the disturbing condition. Second, he reasoned that the interruption would have to be bilateral, since in other dual organs of the body, the activity of one can be taken over by the other in the event of loss.

As so often happens with speculations that lead to important practical advances in medicine, the original theory upon which Moniz based his approach has not found wide confirmation. A great deal of progress has since been made in broadening and increasing the knowledge of frontal lobe functions, but today, with more understanding, scientists are actually less set in their ideas of the frontal lobes than they were ten years ago. The bilateral emphasis by Moniz was in effect more important than his concept of function; it has been demonstrated that the loss of only one frontal lobe has no significant effect on personality performance.

Many observers are now inclined to ascribe the benefits derived from frontal lobe surgery to a reduction in the emotional charge attached to abnormal ideas. They believe the success of the operation lies in the interruption of the pathways between the frontal lobes and the hypothetical center of emotion located elsewhere in the brain, presumably in the thalamus. With the reduction of the emotional energy from the thalamus, delusions and hallucinations tend to lose their importance, bizarre mannerisms and motor manifestations to recede, and preoccupation with physical complaints to fade into the background.

Thus, although the fundamental disease process seems not to be directly altered by psychosurgical procedure, the patient is relieved of abnormal emotion attached to morbid ideas, and his attention can be diverted to more realistic concepts and more constructive activities. In the opinion of most authorities, some type of unpleasant and disturbing emotion in the reaction pattern of the patient is an important criterion for selection of patients.

The technique originated by Moniz, and since improved by him and others, has come to be called, in this country, the prefrontal lobotomy. There is still a long way to go in refining operative procedures, and in improving methods of selecting patients, but the field has advanced far beyond the point, in the early days, when patients were chosen on the basis of what might be called negative selectivity—the operation performed as a last resort when the patient was so far gone that he had nothing to lose anyway. Certainly, if too much of the personality has deteriorated in the course of the disease process, little can be expected from psychosurgery; it is not a restorative.

Today most observers see the best outlook for prefrontal lobotomy in long-standing depressive illnesses, particularly the involutional type, and in incapacitating obsessive-compulsive neuroses. Also, certain schizophrenic patients, especially the catatonic subgroup, have benefited from the operation. Contraindications for lobotomy are present when the emotional tone has become chronically flattened (the operation would only "flatten" it all the more); and the advisability of operation is also questionable in those cases where antisocial traits were evident in the previous personality.

The surgical technique employed varies with different practitioners. Moniz adapted an instrument called the leucotome (whence his use of the word "leucotomy" for the operation) with which cores could be cut in the white matter of the brain and left *in situ*.

Freeman and Watts, who first introduced the operation into this country, have used a blunt knife to section the white matter at the posterior end of the prefrontal lobes, making their incision through a trephine opening in the skull in such a way as to sever the main portion of fibers between the thalamus and the frontal lobes.

Following the original lobotomy, a number of new techniques have been developed in the effort to learn more about what is actually cut in the brain, and to control more precisely the site of the incisions and the amount of tissue sacrificed, the latter logically being the minimum necessary to produce the desired clinical result.

After the original "closed" technique of lobotomy was introduced, Lyerly, an American, developed the "open" technique, in which the skull is opened by turning down a bone flap. Many of the larger clinics in this country are now using the open technique.

The topectomy was originated by Pool, who felt that to obtain beneficial results in psychotic patients, it might not be necessary to disconnect the frontal lobes as extensively as in the "standard" prefrontal lobotomy. The topectomy is also a bilateral procedure, but instead of the cores being left *in situ,* certain areas of the cortex in the frontal lobes are actually removed. So far, the therapeutic effects of topectomy are similar to those of lobotomy—that is, relief from emotional pressure, tension and anxiety. Further, there seems to be a minimum impairment of social, moral, and intellectual performance.

Among other techniques being advanced, Freeman is pioneering in what is called transorbital lobotomy, a less drastic method that he recommends for early cases of mental illness; Wycis, Freed, and Spiegel have developed the thalamotomy, which involves partial electrical destruction of the main connecting station from the prefrontal lobes; and Scoville is doing preliminary work in selective cortical undercutting, in which certain areas of the cortex are separated, but the cortex remains in place, a technique that is based on experimental work in Fulton's laboratory at Yale. Also, a group in England is undertaking to outline areas for neurosurgical attack according to symptomatology.

All this investigation is in search of techniques to reduce time of treatment and hospitalization, at the same time achieving maximum relief from mental suffering, with minimum social and personality disturbance.

After prefrontal lobotomy, a change in the personality of the patient is evident, even on the operating table itself when the operation is performed under local anesthesia. The voice shows a change in emphasis and inflection, and the patient appears oblivious of the emotional troubles that had been harassing him so severely a short time before. At this time, immediately after the operation, when there is a reduction in emotional tone, when the behavioral display is superficial and improper habits are not yet ingrained, it is essential to bring into play the right kind of influences in a favorable interpersonal and material environment. Prompt rechannelization of the haphazard reduced emotional energy at the disposal of the personality after the operation determines to a great degree the eventual social adjustment of the patient. So it is that psychosurgery cannot stand alone; it must be applied in close correlation with other psychiatric treatments, including, especially, re-educational therapy.

Postoperative patients may present a special problem. Because their new personality components are carelessly assembled during the period immediately following operation, they should not

be exposed to old psychotic patterns, nor is it desirable that they mingle immediately with convalescent patients. They should be assigned to special postoperative groups, and participate in a program of activities shaped by life outside the institution and especially designed to provide the stimulation, encouragement, and re-education necessary at this time.

Although some psychiatrists recommend early removal of the patient from the hospital back into his home environment, others, including the writer, are convinced that a premature return to the family, to the environment in which the illness originated, at a time when the patient presents so many behavioral and nursing problems, may easily compromise chances for an optimal result. The well-controlled hospital environment, with especially trained personnel, is best suited to the task of retraining.

After a number of years of careful investigation and work in psychosurgery and postoperative re-education, the Institute of Living has found it advisable to construct a special psychosurgery unit where operating facilities, postoperative training facilities, and residential quarters are all housed under one roof. Within seventy-two hours after the operation, and sometimes the following day, the patients are up and engaged in activities. This postoperative retraining program, like other work in this field, is still in its pioneer stages, so modifications and refinements must be constantly envisaged. Current results are nevertheless highly encouraging.

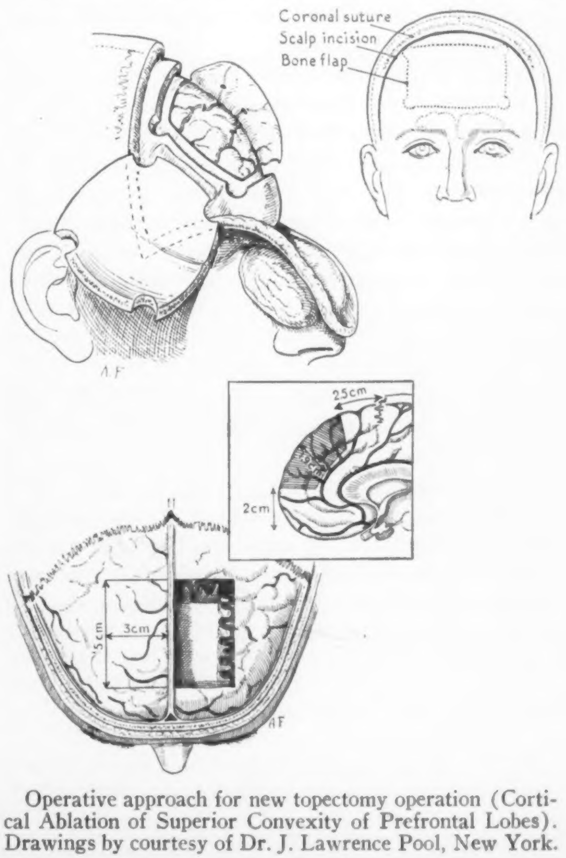
Broadly speaking, the program embodies adequate encouragement and personal attention, stimulation and motivation, and consistent social pressure. The postlobotomy patient, even more urgently and conspicuously than any other psychiatric patient, needs a consistently active routine throughout the day, and it is of paramount importance that the re-educational program for this group be geared at all times to the characteristics and capacities of the patient, with provision for gradual expansion from relatively simple individual activities to increasingly complex projects and group activities.

The post-lobotomy patient tends to think in concrete terms. Most studies indicate that intelligence is not measurably impaired by the operation; the difficulty lies more in a reduced capacity for prolonged attention, a lack of initiative, and inferior planning ability, all of which have been rapidly improved with proper training.

In the early postoperative stages, the patient is extremely suggestible and susceptible to immediate external stimuli, but his span of attention is short. He must be reminded to continue the task which confronts him, even where meals are concerned. His foresight and his conception of time suffer, and he may idle away the day if permitted to do so. Once he gets busy, though, he may do very well at a variety of more or less complicated tasks. To accomplish this, most patients need prodding and will take it in their stride. Guidance must be firm and consistent. The patient must be kept occupied in constructive individual activities or in group situations favorable to the development of initiative and socialization.

The re-educational program extends throughout the day, with individual and group activities variegated and flexible. Great emphasis on grooming is important in the early postoperative period: the men have regular barber appointments, and weekly appointments are made for the women with a beautician for hair-styling and instruction in the art of facial make-up.

With improvement in individual performance, the patient is encouraged to participate in group activities and to assume responsibility for such participation. For women there are special classes in home economics and in other activities germane



Operative approach for new topectomy operation (Cortical Ablation of Superior Convexity of Prefrontal Lobes). Drawings by courtesy of Dr. J. Lawrence Pool, New York.

to their domestic pursuits. Men have special activities in the field of physical education and sports. A coed social hour is provided daily in the form of informal dancing and games, and coed classes are given in calisthenics, rhythm games, and group singing. Each patient frequently has a two-hour motor ride to get him in touch with the community, and to vitiate any institutionalized pattern that may have eventuated from his past hospitalization.

In favorable weather, picnics are arranged for those patients who will benefit from them. By appointment, patients may purchase articles in the campus shop, where they calculate prices and count out their money for the transaction. Their interest in reading is encouraged at the campus library, where current periodicals and best sellers, fiction and nonfiction, are featured. Classes in vocational subjects, avocational pursuits, and physical education stimulate interest in personal performance.

Activities, as cross-sectioned above, complement the psychotherapeutic work carried on by the psychiatrist with the individual patient, and thus are directed primarily toward the goals emphasized by the Institute for all patients in the re-educational phase of its psychiatric program—goals of vocational orientation, avocational outlets, satisfactory social and recreational relationships, and good physical educational habits.

Within the psychosurgery unit, groups are set up on the basis of general behavior and ability to cooperate with others, and the individual patients are promoted as they reach a higher level of achievement. The next step for the patient is to take his place in the regular convalescent group in the hospital. Finally, with the abatement of symptoms of the original illness, the control of the psychosurgical phenomena, and the achievement of a sense of responsibility consistent with everyday life outside the hospital, arrangements are made with the family for taking the patient home and getting him to work immediately in a job suitable to his resources.

The training period may be as short as three months; for those who have been seriously ill before the operation, it may be nine or twelve months. During that time, behavioral improvement usually accrues in consistent fashion, though in some cases considerable fluctuations occur for an extended period. Some patients make significant progress for a year, and others continue to improve for several years.

In the tabulation of results from psychosurgery, it is seen that of a group of patients who had been previously regarded as hopeless and destined to spend their lives in a mental hospital, between 30-50 percent have been re-established outside the hospital on a self-sustaining basis; a percentage of the remainder have been established outside the hospital on a semi-independent basis; and, with a few exceptions, the rest have been materially improved over what would have been their destiny without the operation. It is evident that in skilled hands, the danger to life and of aggravating conditions is negligible.

As with all new treatments that prove at all successful, psychosurgery brings with it the danger of overenthusiasm for its effectiveness. Unsettling to unsophisticated doctors and laymen uninitiated in the field are the near miracles, in which psychosurgery has helped to relieve patients who have been desperately ill for long periods of time and who have not benefited materially from other forms of treatment; but the pioneers in the field, who hope perhaps the most conscientiously for its bright future, are the ones first to advocate the prudent viewpoint toward this powerful tool that has been placed in the hands of the profession. Occupying a somewhat spectacular place in psychiatry, psychosurgery is now advancing through increasingly precise criteria of selectivity, more refined operative procedures, and improving techniques of retraining and rehabilitation, all of which have seen progress in the past year.

These problems are receiving the attention of some of the most astute, most brilliant scientists in the world. In 1935 Dr. Egas Moniz cast a ray of light that is today brightening the hopes of the mentally ill, who are still the greatest public-health problem facing the country. Through the studied development and careful application of psychosurgery in conjunction with psychotherapy, tomorrow promises to dawn on an ever-brighter future in the treatment of mental diseases.

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BOOK REVIEWS

THE GIFT OF TONGUES

The Miraculous Birth of Language. Richard A. Wilson. 256 pp. $3.75. Philosophical Library. New York.

BEFORE being certified as educated or eligible for the franchise or for any scientific, religious, legal, or civil employment," says Bernard Shaw in his preface to *The Miraculous Birth of Language,* "I should like everyone to be examined in this book." Such an ardent endorsement is perhaps prompted particularly by Professor Wilson's Foreword, emphasizing an anti-Darwinian organic hypothesis which assumes that purposive mind is a basic and permanent element in the world and thereby expostulating what we already know to be a pet viewpoint of Shaw's. Actually this organic hypothesis, which Wilson finds more synthesizing than others, has little to do with his treatment of language. It would therefore be unfortunate if his acceptance of such a view prejudiced consideration of his other ideas, ideas set forth by himself as follows:

> I have treated language as one step or cycle in the general evolution of the world: the emergence of conscious mind in the world and the new problem that emerged with it; the birth of language in answer to this problem; the materials from which language was made; the metamorphoses it underwent in reaching its final form; its structure in relation to space and time; and its unique character among other phenomena of the world.

Professor Wilson approaches his objective by a twofold method. First, he critically evaluates various theories concerning the origin of language, beginning with the Hebrew and continuing through those of Herder, Kant and Darwin, giving a rather detailed summary of the last and presenting arguments for its inadequacy. Second, he offers his own view emphasizing the emergence of consciousness in human beings which freed man intellectually, via the process of language, from space and time and set him apart from other animals.

An animal below man—the dog, for example—has no sense of time prior to his own birth or subsequent to his own death; therefore, he has no consciousness of any other dog's life at any other time. The same applies to space, which for the dog consists merely of local space. Man, on the other hand, has no difficulty visualizing limitless stretches of space and time beyond the range of his senses. This ability is due to the difference between the mind of man and that of other animals, a difference reflected in man's speech—in fact, creating speech. As Wilson puts it,

> If the animal does not conventionalize sound so as to differentiate one sound explicitly from another, and to multiply their number as man has done, it would seem to follow that he does not *in his mind* differentiate one object from another in space, or one event from another in time. Man's power of explicit mental differentiation was what brought human language into existence.

Ability to differentiate prompted man to fashion a tool permitting the mental representation of the things differentiated. Thus, through the definite shaping of natural sounds, the conventionalization of their meaning, and their multiplication as needed, man has elaborated a language structure by means of which he can translate the "actualized space-time world of nature into the new-born space-time world of mind."

The analysis of this process for both oral and written language provides not only stimulating reading but also a better understanding of the tool used by so many and understood by so few. For the scientist, especially, it is well to consider that language—written language—is less susceptible to the never-ceasing detrition of time than any other product he may fashion. Science, as the refined and accumulating awareness of the universe, must have written language as a bulwark against the evanescence of time.

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OUR IMPERILED RESOURCES

Road to Survival. William Vogt. xvi + 355 pp. $4.00. Sloane Associates. New York.

LEARNING on almost every front has been a war as much against the unknowns as against the trees obstructing our view of the forest. In medicine, we have learned and moved from the diseased organ, to the disease, to the patient; eventually we must move to the patient in his total environment. In education we have moved from compartmentalism, to the individual disciplines, to a synthesis of disciplines; eventually we must weave the whole cloth of integration. In statecraft we have moved from the city-state, to the nation, to the United Nations; eventually we must move to world government.

In recent years we have given much lip service but have yet to learn the imperative in the interrelatedness of man and man and of man and his physical environment. Writing readable, sound, and persuasive science, Vogt warns that unless we learn these facts and do something about them, our chance of survival is about as good as the chance of a procession of saints' images halting the plague.

Drawing upon the best of the technical studies and his own observations and experience in traveling from country to country, Vogt arrays incontestable evidence on the plunder of our all-too-limited natural resources,

on the mismanagement of our food production, on the maldistribution of populations, and on many other questions. The adductions and outlook are dire—unless, as Vogt suggests, we—and he means all of us, you and me, and not only industries, farm combines, and governments—do something now, not next week, next year, or ten years hence, to restore our natural resources and control our rampant population growth.

You may not agree with all of Vogt's thesis or with the means of achieving his recommendations, but you will come away from this book impressed with the importance and urgency of its message.

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FISH STORY

A Study of Fish. Chapman Pincher. 343 pp. Illus. $4.00. Duell, Sloan & Pearce. New York.

THE author, in his preface, states that this book is intended to present facts about the structure and behavior of fish in a way understandable to the angler, naturalist, and general reader. Technical terms have been replaced wherever possible by simple words.

The subject matter treated is supposed to cover the field rather fully. There are 22 chapters, some of which are: Smell, Taste and Touch; Hearing, Balance, Current Perception; Blood, Breathing and Excretion; Fins and Locomotion; Hormone Glands; Breeding; Fish Migration; Scale and Scale Reading; and Pollution. The book covers practically the same ground as Norman's *History of Fishes*; but, whereas Norman thoroughly knew his subject and recorded it accurately, this book predominates in gross errors in statements of facts, indicating that the manuscript could not have been carefully checked by a professional ichthyologist. Much of the terminology was probably coined by the author and is not used in the fields of zoology or ichthyology.

By way of illustration of a few of the most numerous and most unpardonable errors, I cite the following: Page 5, Figure 3, shows behind the head the designation "spine," and on page 6 it is defined: "Embedded in the muscles of the dorsal half of the fish is the bony spine, which peters out behind but in front is joined to the skull." This "spine" probably refers to the vertebral column, or backbone. Page 8, Table 1, gives a primitive type of classification for fishes. One section is headed "Ventral fins set well back," under which are listed "eels, morays, conger;" these of course lack "ventral fins." We are told, page 12, that "The goldfish has taste cells scattered through the surface layer of its skin." It would be interesting to know the source of the statement that "Brown's killifish was reported from water at 128° F." We note in Figure 52 (of an eye of a fish) that the blind spot is labeled "special sensitive spot." Figure 73, of a gurnard, shows the free pectoral soft rays as "spines." Page 94, Figure 92, has the spiracle of a skate designated as "breathing hole." Page 105, Figure 99, and page 150, Figure 114, show the intestine as "food tube." Page 210: "A few fish, like the hagfish, and the sea perches of the genus *Serranus* are said to be truly hermaphrodite. . . ." On page 218, in regard to fertilization of eggs, the author says: "In most cases the revolving sperm bores a hole through the egg wall." There are dozens of other errors.

The majority of the drawings are most elementary, crudely done, and highly inaccurate. Legends in too many instances substitute absurd names for already well-established terms. The list of common names followed by scientific names is a fairly good one as far as it goes, but I wonder what the "South American mud minnow," page 259, might be!

Publishers certainly have a responsibility to the public and should ask competent advice before publishing such books. I could not recommend it for schools nor for the angler, naturalist, or general reader.

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A NATURALIST'S LIFE

Ant Hill Odyssey. William M. Mann. 388 pp. Illus. $3.50. Atlantic-Little, Brown. Boston.

DR. WILLIAM M. MANN, Director of the National Zoo, writes, as he talks, in a refreshing and colorful manner of his childhood and student days up to the time he accepted his first permanent position as an entomologist in the U. S. Department of Agriculture. From early childhood "Bill" has been interested in collecting insects and all sorts of animals. Warm-hearted and generous, he has liked and been liked by people all over the world and in all walks of life.

Although he is a world authority on ants and the insect guests that live with them, his interests have covered the whole field of natural history. As a young naturalist Mann found that he could not properly study his specimens without a formal education. While a student at Washington State College and at Stanford and Harvard Universities, he made many friends among distinguished scientists.

A collecting trip in Arizona for beetles—financed by a wealthy expert—led to more important trips to Brazil—with the Stanford Expedition—to Haiti, Mexico, the desert of Arabia, and finally to the Pacific Isles. All sorts of odd insects and animals were collected on these trips; many, being new to science, have since been described by Dr. Mann and other scientists.

In Haiti, Mann had a Presidential passport, but it was his own personality and good sense that enabled him safely to penetrate inaccessible regions of this land of voodoo. Among the cannibals of Fiji, where "human meat is sweeter," and in the Solomon Islands, he traveled in safety through the interior and made friends with the natives. Later some of his former

companions were killed by villagers. Exposed to poisonous snakes, wild animals, and disease-carrying insects, Mann's only casualty was a severe case of malaria.

Full of human interest anecdotes, descriptions of wild countries, people, and natural history, this book is an inspiration to both layman and scientist. Bill's friends will look forward hopefully to additional books that will describe later expeditions to South America, Africa, and Malaya, as well as his experiences as a circus fan and as Director of the National Zoological Park.

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STANDARDS OF MEASUREMENT

The Metric System of Weights and Measures. The National Council of Teachers of Mathematics: Twentieth Yearbook. Compiled by the Committee on the Metric System, J. T. Johnson, Chairman. xiv +303 pp. Illus. $3.00. Bureau of Publications, Teachers College, Columbia University. New York.

THIS "Yearbook of the National Council of Teachers of Mathematics" gives a survey of the history, nature, and advantages possessed by the metric system, as supplemented by reports on the use of metric units in this country by some sixty individuals in different occupational fields. The general contention is made that the substitution of the metric system for the common one would give greater speed and accuracy in computation, make possible a freer interchange of knowledge and goods between the peoples of the world, and benefit education by the substitution of decimals for common fractions.

The work of the Committee has been, quite largely, one of compilation, with the contributions accepted for inclusion coming from those who are convinced that the best interests of America would be served by a more universal adoption of the metric system in this country. The writers are, however, not in agreement as to the methods that would be effective in bringing about the change. Thus we have such comments as the following:

> Another course is to finance and carry on propaganda for influencing the people and Congress. This might result in radical action (page 76).
>
> Any attempt to force the adoption of the metric system by mandatory legislation . . . would end in fruitless controversy (page 218).
>
> This should be approached by starting out with our public school system (page 94).
>
> Another basic approach would be for each one of the major industries to make a thoroughgoing study of its particular problems (page 220).

The compiled material is presented in four sections. The first two emphasize the history of the metric system and the present usage of that system in this country. The third is in the nature of a scrapbook, which has gathered together such items as scripts for school radio programs, newspaper accounts of teachers' meetings, reports on talks before clubs, and resolutions passed by clubs or other groups—all urging the adoption of the metric system in this country and the elimination of the common units. As might be surmised, these contributions are not entirely free from overstatement and ballyhoo. The final section carries as its subheading, "Methods of making the change to the metric system, both in general use and in education." The articles included are, however, more diverse than the heading would indicate. Three are by members of the Yearbook Committee and deal with schoolroom situations; all of these are excellent. The first of this group follows a line at variance with the rest of the book in that it scarcely mentions the metric system, giving its attention, instead, to methods of presenting decimals in the arithmetic classes through the decimalization of common units.

For the book as a whole one is apt to note adversely the brief and ineffective Foreword and the blithe disregard of significant digits in such problems as those of pages 54–56. A more drastic criticism would be made of such material as that on pages 81 and 82, which purports to give the history of the common units. The entire discussion is historically unsound. That it ignores the standards of length and weight placed in the Exchequer by Edward III and used officially by all England for two centuries before the time of Elizabeth is certainly inexcusable.

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ZOOLOGICAL SUPERSTITIONS

Animal Facts and Fallacies. Osmond P. Breland. xvii +268 pp. Illus. $3.00. Harper. New York.

ANY teacher of biology or zoology, as well as natural-history museum workers, will testify to the appalling amount of misunderstanding and superstition that clutter up the average person's mind in respect to animal life. Professor Osmond P. Breland, of the University of Texas, has taken time out to record and correct some common misconceptions about animals. He has likewise included numerous items about special habits and idiosyncrasies of certain animals, such as the possible danger of shock from electric eels, the speed of birds, chewing of the cud by cows, and the delicate question of how porcupines mate.

Breland has arranged his sections on mammals, birds, fish, reptiles, and amphibians in question-and-answer form for the convenience of the reader. The work, however, cannot in any sense be considered seriously as a reference text; in the words of the publisher, "This is not a guide book."

Even though much worth-while information may be found between the covers of *Animal Facts and Fallacies,* one takes away the impression that the author has not come fully to grips with his undertaking. For one thing, the absence of scientific names will surely

prove disconcerting to readers who intend to seek additional data on the life habits of the animals they read about. Although the so-called jointed snake is admittedly not a snake, it cannot be dismissed simply as a "glass-snake lizard." The unfortunate and overall result of Dr. Breland's efforts to avoid being technical has been to swing to the opposite extreme, that of oversimplification. Chapters such as A Fish Said to Raid Hen Houses, Frogs that Fly, and Some Frogs that Eat Alligators have relatively little value in a book of his type, and they should not have been included.

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WORLD PROBLEM

Plenty of People. (Rev. ed.) Warren S. Thompson, xiv + 281 pp. Illus. $3.50. Ronald Press. New York.

JUST 150 years ago Malthus published an essay on population in which it was shown mathematically that human population, increasing as it does by geometric proportion, will sooner or later outstrip the possible increase of food production and present a prospect of starvation or the alternative of restricting the birth rate. For nearly a century this crisis was looked upon as lying in the indefinite future. There were still millions of acres of fertile land to be brought under the plow. Especially was this true of the United States and other Western countries where a rapid increase of population was taking place, yet the teeming populace lived better than had their ancestors.

Thus the evil day, when the pinch of hunger might be felt on a grand scale, was postponed, or lost to view. But as the reserve of unused land dwindled toward the vanishing point and the world's population continued to grow by 20,000,000 or more a year, the ghost of Malthus walked abroad again, and to many students of human affairs the menace of overpopulation seemed not far off.

For years Thompson has taken a leading part in the study of this problem. The voluminous census figures and other statistical data from all parts of the world have been thoroughly combed for information on the actual facts of birth rates, death rates, and resulting population growth or decline. Taking into consideration racial differences, conditions of sanitation, climate, disease, economic and social standards and customs, religious taboos, family traditions, and "opinion climate," or attitude toward childbearing, he finds that

> even the most sanguine hopes of science and industry for increasing the means of living will be inadequate to supply more than a meager existence to our increasing numbers for more than a few years unless birth control becomes the rule in all the world. There can be no rational hope of a decent life for all mankind if birth rates remain at more than about one-third of the physiological maximum in any considerable part of the world's population.

The available mathematical data used to buttress these conclusions have, I think, been marshaled with care and fairness. In the analysis and interpretation of the statistics there is some difference of opinion among demographers on certain moot points, such as the effect of immigration on the growth of population in the country of adoption. Among the numerous factors involved in his discussion of population, perhaps special mention should be made of the author's treatment of the immediate and aftereffect of war upon the military and civilian populations, of internal migration, of minorities, of epidemics, industrialization, prosperity, depression, changes in age distribution, use of contraceptives, and various means adopted by different nations for increasing the birth rate, including marriage loans, bonuses for large families, remission of taxes, police drives to reduce abortion, and birth control.

Thompson stresses the importance of the psychological factor in determining the size of the family. The desire for ease and luxury and the unwillingness to make personal sacrifice for children may be more decisive than considerations of economy and security. If the desire for a biological stake in the future through the continuation of the family is weak, perhaps it will be difficult to bring pressure to bear either for more or fewer children. At any rate, the author makes a good case for the proposition that the citizens of every country should more keenly appreciate the importance of maintaining a population of the right size and quality and of recognizing that a reasonable adjustment to the environment, though neglected at present, may soon become imperative, and that the matter cannot be put off indefinitely.

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